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EXTRAIT DE LA PRÉFACE

..... En résumé, analyses approfondies de tout ce qui paraît d'important en psychologie, tables bibliographiques développées, dessins d'appareils nouveaux, mémoires originaux, et revues générales où des questions à l'ordre du jour sont traitées depuis leur origine jusqu'à leur état actuel, tels sont les principaux éléments contenus dans cette Année psychologique.

Nous espérons que notre publication rendra service à tous ceux qui s'intéressent au mouvement des idées, — aux professeurs de philosophie qui désirent tenir leur enseignement au courant de la science, — aux médecins et aliénistes qui veulent appliquer les méthodes psychologiques et leurs résultats à l'étude des malades — aux pédagogues enfin qui croient à l'utilité d'une pédagogie expérimentale, fondée sur l'étude méthodique des enfants.....

TABLE DES MATIÈRES

1re PARTIE

Compte rendu très complet des différents travaux de psychologie parus en tous pays pendant l'année 1894; diagrammes, figures, tables; les analyses ont été faites de manière à dispenser de recourir aux sources. Les principales questions traitées sont : 1° Le système nerveux (récentes recherches de Ramon Cajal, Viallet, Mosso, etc., etc.). 2° Les sensations de la vue (recherches de Bosscha, Hess, Titchener, Hess et Pretori, Hillebrand, Kirschmann, Kœnig, Kries, Hering, Vitschau, Stern, Marbe, Wertheim, etc., etc.). 3° Sensations de l'ouïe (expériences d'Ewald, Exner, Wundt, Schäfer, Hermann, Münsterberg, etc.). 4° Sensations du toucher et des autres sens (expériences de Galton, Bart, Dehn, Dresslar, Krohne, Riley, Preyer, Scripture, Segsworth, etc., etc.). 5° La mémoire, l'association des idées, la paramnésie (recherches d'Armstrong, Bigham, Binet et Henri, Bourdon, Kirkpatrick, Müller et Schumann, Münsterberg, Schneider, Ladd, Warren, Zaborski, Lalande, Dugas, Le Lorrain, Lapie, etc.). 6° Association des idées (Bergström, Calkins, Howe, Jastrow, Smith, etc.). 7° L'attention (Eckener, Lange, Lehman, Marbe, Münsterberg, Pace, etc.). 8° Le sens du temps et le rythme (Bolton, Neumann). 9° Psychométrie et Psychophysique (Bliss, Bryan, Cattell, Hill, Witmer, Münsterberg, Seashore. etc.). 10° Sentiments (Bourdon, Witmer, Pace, Ribot, etc.). 11° Le raisonnement, la volonté, la personnalité. 12° Les illusions, les hallucinations, les rêves, l'audition colorée. 13° Le plaisir, la douleur, l'esthétique. 14° La psychologie des enfants et la pédagogie. 15° La psychologie pathologique. 16° Les nouveaux traités de psychologie. 17° Questions philosophiques.

2me PARTIE

Index bibliographique de plus de 1200 numéros, contenant l'indication de tous les travaux parus en 1894 et intéressant l'histologie, l'anatomie, la physiologie du système nerveux de l'homme et des animaux, la pathologie mentale et nerveuse, la psychologie, la philosophie, la morale, la pédagogie, la criminalité, la psychologie des enfants.

Cet index bibliographique ayant été conçu dans les proportions les plus larges ne manquera pas d'être un utile instrument de travail à une foule de chercheurs.

3me PARTIE

Publication in extenso d'articles et de mémoires inédits avec planches, graphiques, tables, et représentant la majeure partie du travail du Laboratoire de la Sorbonne. Nous citerons: 1° Les recherches de MM. BINET et PASSY, sur la psychologie des auteurs dramatiques (DUMAS, SARDOU, CUREL, PAILLERON, A. DAUDET, E. de GONCOURT, F. COPPÉE, MEILHAC, etc. etc.). 2° Les recherches de MM. BINET et V. HENRI, sur la mémoire des mots, des phrases et des idées chez les enfants, etc ... 3° Les études de M. FLOURNOY sur l'audition colorée, sur le milieu psychologique, sur les illusions de poids. 4° La vaste enquête de M. DELABARRE sur l'état actuel de la psychologie en Amérique. 5° Des recherches expérimentales de phonétique de M. WEEKS, etc. etc.

4me PARTIE

I — Variétés contenant l'indication des observations, expériences, instruments nouveaux pouvant servir à la psychologie.

II — Nécrologie.

VOL. II. No. 3. MAY, 1895.

THE PSYCHOLOGICAL REVIEW.

PRELIMINARY REPORT ON IMITATION.[1]

BY PROFESSOR JOSIAH ROYCE,

Harvard University.

In calling these few notes a Preliminary Report, I have deliberately wished to ask for all the indulgence which the phrase itself can properly invite. I have none but tentative considerations to present. I mean to tell something of the mere plan and programme of a research which I have ventured to begin, but which I cannot hope ever rightly to finish, concerning the processes that enter into the structure and growth of our imitative functions. In making my statement, I shall first be led to speak, perhaps at far too great a length, about the difficult problem of the possible classification and definition of the processes which we can call, with more or less right, imitative. I shall do so because my experimental work, later briefly sketched, has already suggested to me, not only the need, but at least one motive of such a classification. Then I shall very briefly indicate the first beginning of an experimental study of some simple imitative processes which I have been prosecuting only since October 1, at Harvard, under the guidance of my colleague, Prof. Münsterberg. If my little sheaves are, so far, very naturally lean, they may still suggest the fact that in this field the harvest is plenteous, whatever you may think of any of the laborers.

[1] Read at the December meeting of the American Psychological Association, at Princeton.

I.

If we ask ourselves: 'what is the definition of Imitation?' we soon find that any effort to separate imitative motor functions, in ourselves, from those which are not imitative, or imitative conscious processes from those mental processes which are not imitative, is a very difficult thing. Aristotle, who first undertook to define the psychological category of imitative mental processes, and of their correlative expressive functions, was quickly led to extend the term imitation until it came to include the most original productions of that poetic art which he himself called more philosophical than history. No student of the subject can easily avoid a wide extension of the category in question. Prof. Baldwin, in defining imitations (from a general biological point of view), by the very wide characterization which identifies them with 'circular' reactions, or with 'motor processes that tend to reproduce their own stimuli,' has seemed, I suppose, to many of his readers, to have made the concept of imitation far too inclusive for psychological convenience. All acts of sensuous attention are, of course, such circular motor processes. Again, is the insistent brooding of a mourner, as such, an imitative process? Yet it, of course, involves usually many circular reactions. Yet, from another side, there are facts that Prof. Baldwin himself has noted in passing, and will regard as familiar, but that would seem to tend to make even a still wider definition than this one desirable. In the laboratory experiments, later to be mentioned, I ask my subjects to listen to a rhythmic series of taps (made with an electric hammer), and then to reproduce this series by means of an electric key. Now, suppose that a subject persistently guides himself, in this process, as one of my subjects has often done, and as, no doubt, many people would do, by first, more or less deliberately, translating the series of taps to which he listens into a visualized image of pencil strokes arranged at intervals on paper, or of points marked off on a line; and suppose that he hereupon regularly makes his key-imitation by translating back from the visualized space intervals, and perhaps from the muscular feelings

of making the visualized pencil strokes into those muscular movements which reproduce the sound at the key. I suppose we should all agree that here not only is the final reproduction of the original series an imitative process, but the translation into visual and muscular terms, which serves as an intermediate instrument, is itself already an imitation. But, if so, the intermediate stage, which, be it noted, was not, in the subject noted, a spontaneous accidental association, but which was the gradual and habitual outcome of all the motor processes of his careful attention, and which arose as an incident of his deliberate effort to reproduce what he heard,—this intermediate stage is surely not itself the result of a function that reproduces its own stimulus, but of a function that produces, in image form, contents which are not those of the stimulus, but which have relations similar to those presented in the regular stimulus. But that imitations are thus often translations, reproductions which do not even mean to bring back the original stimulation, but which do mean to interpret it, by setting over against it its illumining counterpart in terms of some other set of stimulations, this, at least, on the highest levels of consciousness, is a commonplace. Aristotle already told us of art as intending to imitate life by producing before our eyes something that is in pretty marked contrast to life, e. g., an heroic tragedy. My subjects, at the key, as you will later see, find themselves both voluntarily and involuntarily doing a good deal of this same general sort of thing,—their imitations being often essentially interpretations, just as a translator imitates the original text precisely by meaning to write out, not the words which he gets as a stimulus, but the words of another tongue, which may, as faithfully as possible, serve the same ideal purposes. But in the same way, apart from the special motives of the translator, we all have very deep-set habits of imitating the sensations of one sense by means of deliberately recalled images belonging to another sense, and so, by motor reactions that tend to reproduce the stimuli to which these latter images correspond.

Thus the effort to define imitation, whether by wide or by narrow phrases, is at every point met with pretty decided

difficulties. Ignoring, as far as possible, any but the psychological point of view, I may, however, venture at this stage to suggest some of the more prominent classes of imitative functions, and of functions more or less obviously related to imitation, classes such as I myself have been led to distinguish. Whether any convenient generalization, as to the extent of the word imitation, is possible, we may then briefly consider.

To us all the word imitation first suggests motor functions, such as those of the child that struts about as a soldier, or that runs on all fours as a dog, or that learns to talk. Such functions are very numerous. We observe them in many animals, including birds. Their characteristic is that the imitator is more or less clearly aware of a model, and finds his own body more or less able to repeat certain usually extensive and complex movements of this model. This repetition gives satisfaction to the imitator. Imitation of this sort is to be roughly classified as either more or less critical. Sometimes the imitator is content with the roughest reproduction. Sometimes he is cautious, and is watchfully anxious to do precisely as his model does. A mocking-bird, as I at one period often observed in case of a household pet, appears to study with very great care at least some of the series of notes that he reproduces. Some children far surpass others in an early pedantry about the enunciation and use of their words. In any case, meanwhile, the subjective experiences of the imitator are here, at best, only in part identical with those given him in the stimulus presented to his senses by his model. He hears sounds, and replies by sounds, but of course he feels, more or less, the muscular and other organic disturbances incident to the reproduction. He sees the movements of his model. He both sees and feels his own imitative movements, and in all this he feels the latter as his own. In consequence, the imitator usually takes what is often called a decidedly 'subjective' sort of interest in his power to imitate. His activity has thus two strongly contrasted aspects. He watches his model, so far as he watches it at all, with a highly objective faithfulness. So far, his imitation depends upon a theoretical and very self-

surrendering sort of outward scrutiny. On the other hand, he delights in his own imitative powers as his own, i. e., as corporeally interesting events in his own organism, just as even the mocking-bird very obviously does. On this side the activity is, in the popular sense of the word, as self-centred as is eating or catching prey. That is, it is an activity whose conscious aspect involves an interest in inter-organic experiences. And, on this side too, the imitative process, in our children, is a great meeting place about which all sorts of self-considerate and self-conscious interests gather. Thus one sees how highly inter-organic, or subjective, as well as how highly outward-looking, objective, the imitative consciousness in the present class of cases has to be. Hence the enormous fecundity and various outcome of such imitative interest. Vanity and conscience, ideal devotion and flippant mockery, tame subserviency and the loftiest originality,—all these tendencies alike may, and in fact normally do, take root in this fruitful soil, and any of them may grow into the child's later character, and all because he was, in the first place, disposed to repeat the complex motor processes of his models, and so was forced to set off his consciousness of his own movements against his perception of the movements of others, thus emphasizing both his ideas of himself and his ideas of his models, each set of ideas by contrast with the other set. Imitation may thus become, to use the words again in their purely popular sense, the most self-abnegating or the most self-considerate of tendencies, according as, in the end, one or the other of these opposing drifts of attention gets emphasized, i. e., according as one comes to consider rather his own imitative organism or the outside model.

So much for a first and most familiar class of processes defined as imitative. But we all of us extend the word imitation to include those intelligent functions which tend to the voluntary production of external objects resembling certain other objects called the models of the objects produced. Thus, drawing, painting, modeling, building, mechanical skill of all sorts, are universally named imitative functions. In our own cases such functions, as a class, are obviously almost altogether derived, directly or indirectly, from the

functions of the former class just characterized. We learn, namely, to reproduce things, in whole or in part, by first having learned to imitate people. Mechanical skill may early become self-directing. But it is probably always, at the start, socially guided. Psychological complications are, accordingly, here of much the sort as in the foregoing class of cases. Our imitations of objects involve vast numbers of relatively controllable conscious processes besides our perception of the finished products which resemble the stimulating models. And here, too, in consequence, both our relatively objective, or outward-looking, and our relatively subjective, or inward-looking, interests get a correlative development as we learn to imitate—a development which may have the utmost complexity, and the most momentous psychological consequences. On the whole, however, the imitation of things generally tends, as they say, to send us 'out of ourselves,' i. e., outside of our interest in the processes of our own organisms, still more than does the imitation of the mere acts of people. Our imitative deed is transient; but, when we make something by the deed, its product here remains to calm our more anxious or our vainer interest in our own motor processes as such. Hence it is that musicians are more subjective in mood than are architects; and it is easier to be vain about matters of social etiquette than about one's skill as a carpenter, in case one has any such skill; while, to pass to another case where imitation is complicated with originality, nobody can judge his own book while it is in press as he can after it is in cold print and binding before him.

Now, I have laid stress upon the factors present in these two classes of cases of imitation, because I have meant to use them to illustrate the general nature of imitation itself. In these two classes of cases imitation is not merely, as a psychological process, the reproduction of a series of sense stimulations, or of external perceptions by means of a series of motor processes; but it is something still more complex. It is not only a process by which we reproduce one set of data by means of another set of data like the first, but it is also a process by which we get two sets of data whose inevitable

contrasts are as interesting and as instructive to us as their purposed resemblances. We get an interpretation of the perceived model through the imitation of it. On the other hand, to say that imitation, in these cases, is an act whose main motive is to interpret my perceptions by means of my deeds, is indeed true; but of course, so far, the same might be said of all those acts, such as looking, listening, approaching an object, grasping, touching, handling, exploring, in the perceptive field,—of all acts, in short, which involve intellectually valuable motor processes. What, then, is the characteristic feature of the imitative acts in the mentioned classes of cases? Does it not obviously lie in the fact that my interpretation of what I am usually said to perceive outside of my organism, in the external world, is, in the case of these classes of imitations, conditioned upon my setting over against my perceptions a series of motor processes, or of perceived results of motor processes, which in its wholeness contrasts with the other series in the one principal fact that the motor processes, the imitative deeds or their results, appear to me relatively controllable, plastic, reproducible at will, while otherwise the two series are largely similar. When I learn to grasp an apple, the grasping is indeed, once learned, an easily reproducible and so controllable deed, and on suggestion is remembered as such. But when I learn to say *apple*, upon hearing the word pronounced, the act, once in my power, is felt as controllable, but as to result it resembles its model (namely, the word apple, pronounced by my neighbor)—something that concerns not its controllableness, but some of its other characters. Thus, in these cases, imitation is definable, from the psychological side, as an act that interprets an uncontrollable perceptive series by setting over against it a series of experiences that appear to be similar to it in content, but to be also in contrast with it by virtue of their controllableness. Or, again, an imitation is an act that tends to the interpretation of what is beyond my power, or is independent of my movements, by contrasting it with what otherwise resembles it, but is in my power, and is a result of my movements. This feature of imitation, viz., that it accomplishes the aim of throwing light on the

uncontrollable percept by setting the controllable deed beside it, is, I suppose, the principal intellectual function of the higher imitative life. That the light thrown on the processes is throughout relative, that what I perceive outside me helps me to know, by contrast, my own imitative act, as well as the latter helps me to know the former,—this, after what has been said, needs no further illustration. At the outset, of course, we make no clear sundering between what goes on inside our organisms and what we perceive outside them. My point here is, it is our imitation that helps us first to do so by first bringing the mentioned contrast to light.

Now, however, as helping us on to another class of imitative functions, we may note the fact that where I thus use imitative processes to set off or to interpret perceived facts that are outside of my organism, it is not necessary that the similarity between the externally observed and the internally produced processes, between the original and its so-called copy, should have any one established or even desired degree of closeness. I insist, it is often the contrast as much as the agreement between the two that interests us. In every case so far the imitation differs from what it copies by virtue of the associated muscular and affective accompaniments which make the imitation our own, as distinct from what we merely observe without. These accompaniments may involve all the emotions of play. In that case the imitator very frequently wants his imitation to be unlike as well as like its original. One plays in one's own original fashion. Mocking is often more or less consciously untrue to its model. 'This is what you do,' we say to the person whom we mock. But thereupon what we do is only a pretended imitation—an exaggeration, of whose grotesque unreality we then make an ideal. Children surely often do this. The reasons for such action lie deep in the nature of the play motives. The mocking imitation is as imperfect a copy as are often the actions of kittens at play, when compared with the behavior of grown cats that are seriously fighting.

If an imitation thus often sets off our consciousness of the original by virtue of the very contrast that mingles with the similarity, it is plain that we may look to find imitations that

not only by accident, but intentionally, represent one set of sense data in terms of activities that give us data belonging to another sense or to any otherwise contrasted group of experiences. The imitation of a series of sounds by a series of movements involves, of course, as in dancing or in beating time, a vast number of acquired habits of conplex nature. Yet the fact remains that such imitations do both fascinate and enlighten us. This principle of the tendency to deliberate idealization of our imitations, to deliberate deviations from the literal, one finds, then, in the most varied forms, in play, in art, in the far-reaching and deep-seated tendency, very complex in its origin, to translate space-relations into time relations and *vice versa ;* in every form of fondness for what one may call symbolical motor processes, and so, finally, with very momentous consequences, in all those motor processes that are connected with the growth of our theoretical thinking. That our thoughts are, in this general sense, conscious processes by which we constantly mean to imitate the truth of the things that we experience, is perfectly obvious. Equally obvious is the fact that to think experience is to translate it into terms which are decidedly foreign to its character as it comes to us, apart from such ideal reconstruction, and in its first intention. Now thinking accompanies motor processes, abbreviated and truncated and rendered abstract in all sorts of ways, but very obviously and highly imitative in all the cases where we get them in any relatively unabridged form. The gesture language is a case in point. It gives the gesturer trains of experience of a very complex character, which are in a summary and more or less symbolic fashion similar to the primary trains of experience which by his gestures he undertakes to describe. For gestures we who speak have now learned to substitute trains of words, which we follow with an endless chain of attentive processes shifting from one series of images to another. But the series of attentive processes, as it follows now these, now those images, gives us a total inner experience which we call an account of the experienced reality beyond the thinking process. The value of this account we judge by its resemblance, not in detail, but in its total net-

work of related elements, to those aspects of the relatively external experience which our thinking means to emphasize. And yet, on the other hand, how unlike their originals our abstract ideas mean to be. How far is the thinker's imitation from being a mere inner reproduction of the external experience about which he thinks? It is the very contrast which here enlightens us, when it is accompanied by a consciousness of the sort of agreement which we all the time intend. In symbolic imitation the imitative subject means to neglect all of his model except his own chosen aspect of it; and even this aspect he generally means to reproduce in terms of a sort of inner experience which differs from it as widely as the data of one sense can differ from those of another.

So far I have mentioned ordinary imitations of the doings of our comrades, acquired tendencies to reproduce or picture things, and then the endlessly numerous cases of consciously idealized, playfully falsified or symbolically abbreviated imitations of the interesting aspects of things. Is it not fair to call all these manifestations of the imitative tendency? But some one will say, as people have said of both Tarde's and Prof. Baldwin's uses of the term imitation, that to go on in this fashion is in the end to include pretty much all psychical processes in the field covered by the word. If imitation occurs wherever there are relatively inner or organic experiences—*e*, *g*., images or trains of images which, in some aspect, resemble certain relatively external or perceptive experiences—then where can one name an experience involving any images whatever, or any organic adjustment, which will not have something imitative about it? I reply that, if the foregoing classes of cases were all that I had to consider, I myself should be disposed to draw the lines about the class of processes to be called imitative from a purely psychological point of view, in this way: An imitation either is or accompanies a sort of motor adjustment. And, now, what sort? I answer: So far as we have yet gone, an imitation appears as an adjustment that leads to the emphasizing or interpreting of a train of relatively external experiences, by virtue of the fact that the mental accompaniment

of this adjustment is a train of relatively inner experiences (muscular feelings, or images of any sense you please, or affective states), while the similarity of the train of internal experiences to the train of external experiences serves, in the midst of the mutual contrasts of the two trains, to make livelier the consciousness of each series, when viewed side by side with the other. Or, more briefly, an imitation is a more or less conscious motor adjustment that tends to set off a series of given experiences by furnishing from within the conscious counterpart of some one or more of the aspects of the first series—a counterpart which is both like and unlike the original, and whose contrast is therefore often as instructive as its similarity.

Essential to this notion of imitation is so far the fact that the consciousness of the imitator is as truly a consciousness of his adjustment as it is a consciousness of his model. To be sure, at the outset, an infant has no clear idea of himself; but the point is that the ideas of inner and outer thus get clarified. The two must be more or less clearly held apart. How clearly depends upon what grade of consciousness you are considering. Moreover, the model is not a simple sense-fact, like a color, but is always, where we speak of imitation, a complex series of facts. We imitate the complex. We may by mere association reduplicate the elementary, but in that case we have no true instance of imitation. Where association by similarity takes place between a relatively elementary fact of sense and an image, there is no imitation: (1) because one isn't at all conscious of this association as involving what we call his motor adjustment as such; (2) because in many cases the associated elements tend to blend, and not to set each other off; and (3) because by imitation we always mean a consciously complex process of adjustment. Imitation, in the classes of cases heretofore considered, does not mean, therefore, mere similarity of relatively inner and relatively outer experiences, but the similarity of a complex motor series or of its complex result to a complex perceptive series, the conscious interest lying in the antithesis of the two as well as in their mutual support. The two are not merely alike, but each is more or less consciously

referred to the other. The imitation *means* the model, as well as chances to resemble it.

But, if we now proceed one step further, we do indeed seem to meet with functions which an external observer calls imitative, but which apparently do not conform to the foregoing definition. Many of our imitations occur with very little consciousness. We sit when others sit, rise when they rise, yawn when they yawn; follow fashions without any clear intention to do so, and catch by contagion tricks of gesture and facial expressions, as well as states of emotion. Panic-fear, in all gregarious animals, involves functions that seem clearly imitative. Yet here one surely does not mean to observe either the likeness or the contrast between the outer and the inner experiences. In fact, a contagious emotion, such as terror or a violent sympathetic faintness at the sight of another's pain, often seems rather to forbid the appearance of any clear or conscious sympathy with one's fellow as an objectively real person, and one gets lost in one's own feelings even while one is imitative. Yet even here, although the antithesis leaves consciousness, and the relatively subjective series of inter-organic processes and experiences does not help one to interpret the relatively external facts in any deliberate way, it still remains true that we have the one series emphasized by and dependent upon the other; and while the imitator himself does indeed lose sight of any clear relation of himself to his model, the external observer calls this an imitation, and not, like the independent nest-building of two birds of the same species, a mere resemblance in function, because the observer can see what the imitator neglects—the close relation of dependence between the two resembling and contrasting series.

Now I, of course, cannot doubt that, biologically speaking, the tendencies towards a relatively unconscious conformity of an animal's conduct to the conduct of its herdfellows, lie deeper than the more conscious and intelligent sorts of explicitly discriminating imitation which I have so far defined in this paper. But the question still remains as to what it is about these relatively unconscious sorts of imitation which makes them the basis of so much that is later

important for the higher psychological functions. I venture then still to point out that the unconsciously imitative gregarious animal is still going through motor processes, such as place, side by side with various series of his sensory stimuli, a great number of inter-organic series of processes. These processes, on the one hand, extend far beyond the mere adjustment of his sense organs to the stimulation, while, on the other hand, they tend to emphasize these sense stimulations, not merely by repeating them, but by giving them companions which in various ways resemble them, and which therefore make them more effective in leading to further conduct. When chickens suffer from contagious fright, they all repeat the warning cry of the flock. Of what use is the repetition? Each fowl is in consequence warned, not only by his neighbor but by himself, and the inner warning comes not only to the ear, but also through just those motor sensations and affective inner states which accompany this motor adjustment. No other fowl could warn this one as the bird can warn itself. What is heard without already puts each fowl somewhat on the alert. But the inner resonance of the imitative act makes far more impressive this whole experience of danger. So then, even here, an imitative act appears to me to be not so much an act that, in Prof. Baldwin's phrase, tends to repeat its own stimulus, as an act that tends to reinforce, emphasize, signalize, clarify its complex stimulus by adding thereto other and parallel series of internal or organic stimuli, which by their similarity as a series shall support, while by their differences they shall in general supplement, the stimulus in question. This inter-organic imitation and supplementing of one series of stimuli by a series of inner experiences is, as a fact, very naturally connected in similarly organized beings with a behavior that, externally viewed, appears imitative, even when the creature in question is not interested in this imitative character. Simple attention need not, from my present point of view, be regarded as involving imitation, although simple attention does involve a circular reaction which tends to the repetition of its own stimulation. But if attention is supported by the appearance of a series of experiences,

motor or emotional, which are produced through the motor adjustment, and which taken together, run parallel to a series of stimuli and resemble it, thereby emphasizing and supplementing it, then one has an imitative function.

I conclude, then, so far in general, that imitative functions seem to me to be those which tend to emphasize, to support, or on higher levels to interpret, a complex series of sensory stimulations, by producing, as their accompaniment, another series of inter-organic experiences which resembles as a whole the first series, but which involves in general decidedly different activities of the organism, in addition to those of the organs receiving the stimuli. Lower cases of imitative functions already show us the contrast, in the midst of the similarity, between the imitative process and its sensory stimuli. Higher up this contrast is itself made an object of consciousness. Imitation and model are contrasted series of presentations whose relation keeps them apart. And hence it is that, as I myself suppose, imitation is, psychologically speaking, the one source of our whole series of conscious distinctions between subject and object, thought and truth, deed and ideal, impulse and conscience, inner world and external world—in short, of all those familiar and fundamental rational distinctions which psychology has hitherto found so baffling. The contrast between model and imitation is, to my mind, the first appearance in consciousness of that differentiation which in the end makes internal and external experience not merely qualitatively different—as, of course, they more or less are from the first—but consciously discriminated, as at first they seem not to be.

Biologically speaking, I should fancy that imitation might be, in the end, explicable by something even more fundamental still than Prof. Baldwin's circular reactions, viz., by those generally coöperative tendencies which must lie at the basis of all evolution in organisms consisting of multitudes of cells. These involve amongst their number tendencies to direct functional agreement. What occurs in one part of an organism must in general be repeated with variations elsewhere, in so far as the cells of various regions may be of the same general type, and are meant permanently to

coöperate. Such inter-organic repetitions of disturbance (attended with wide contrasts, which run side by side with the functional agreements) we have in all those recently much studied physiological accompaniments of emotion; and in all those phenomena of functional nervous equivalents which attract one's attention in the history of the varying symptoms of many a complex nervous case. Here what happens to one set of cells may tend sooner or later to be represented by a more or less contrasted functional equivalent in some other set of cells. Now these things are not yet cases of imitation. But they suggest a basis upon which imitative functions may have grown.

I said that my few experiments have already, without as yet proving anything, suggested to me the need of some such analysis as the foregoing. The scope of the experiments themselves is comparatively narrow. Yet some of you will perhaps think it already too wide.

I have desired to get some notion of the inner consciousness and of the outer effectiveness of a person engaged in acquiring skill in some imitative process. This process, as I desired it, should be fairly simple, and yet complex enough to involve the coöperation of a number of different habits, interests and forms of attention in the accomplishment of one end. I decided, by Prof. Münsterberg's advice, to choose the process of imitating rhythmic series of taps which were to be made at controllable intervals by means of an electric hammer, and imitated with an electric key by the subjects. In choosing the particular series of taps, I have followed my own choice and responsibility, and must confess that I have tried several rhythmic series that any more experienced psychological experimenter than myself might have easily regarded as too complex to promise any definite results. Yet so far, despite various inevitable eddies in my little stream of experience, I have not been disappointed at the wealth of suggestions that have come to me. I have regarded the so-called time-sense aspect of my experiments as a necessary, but for my purposes a very subordinate, aspect. The nature of the rhythmic consciousness itself comes in my way, but rhythmic consciousness is here only the instru-

ment, not the end. The chief aim for the first has been to get a pretty careful series of records of the facts, and to wait for experience to indicate the best further procedure. The facts collected have so far been objective records of the imitations and a constant series of subjective records written down at once, after such experiment, by the hands of the subjects concerned.

As for the physical mechanism used, a mechanism which, as I frankly confess, better hands than mine generally guide, it is in summary this: On the axle of a kymograph drum wheels revolve armed with platinum points, arranged at pleasure for each rhythmic series as used, the points successively dipping into mercury contacts at the lowest point in each revolution of their respective wheels. The completed contact gives in each case one stroke of an electric hammer. The moment of each stroke is recorded by an electric pen on the kymograph drum, the record itself being controlled by a tuning-fork tracing. Any one rhythmic series having been heard through by the subject (who sits holding, ready for his response, a metallic key especially prepared for these experiments), the subject, at the word 'ready,' repeats the rhythm that he has heard, by making successive connections between the point of the key and a mercury contact beneath. The key itself is arranged so as to be noiseless, or nearly so, in its own movements. At times it is arranged in the same circuit with the hammer, and then the subject, in making the contacts, tries to repeat the very sounds which he has heard and at the same intervals. At other times this connection is avoided, and then the subject makes his imitative contacts with a 'silent key' depending on the inner light only. Every imitative series of key-contacts is recorded on the same drum with the rhythmic series of hammer strokes which was to be imitated. The routine of each experiment is simply that the kymograph is started; the subject, who cannot see, although he does indeed hear the rotation of the mechanism, hears the word 'ready,' and then the series of hammer taps to be imitated. These taps, of course, cannot under these circumstances be made perfectly uniform, owing to the uncontrolla-

ble variations of the hammer's relation to the magnet; but they have no regular emphasis, and subjects learn to ignore the more ordinary of the caprices of the hammer. The rhythm being completed, there is a very brief pause for re-adjustment, when the subject, at the repeated word 'ready,' proceeds to beat off on the key as exact an imitation as he can of what he has heard. He then at once records dated and numbered notes of his subjective experiences during the experiments, and the records are filed.

So far most, although by no means all, of the records have been taken in work upon two rhythms, both complex enough to make the labor of apperceiving and reproducing them with relative exactitude decidedly noteworthy. They have first been learned, then practised upon daily, or as often as possible, their rates being very widely varied, while keeping the relations of the intervals constant. Separate series of experiments upon the estimation of slight changes of rate, apart from imitation, have also been recorded. And a considerable number of records have been taken of the skill of the subjects in independently giving and varying by minimal steps each rhythm after it had been learned. Of late one of the rhythms has been deliberately distorted by introducing irregularly placed new points into three of its more noteworthy intervals; and the vast change thus suddenly introduced into an already well-established series of conscious data has been studied, both objectively and subjectively.

The subjects include at present four women and four men, all of a fair although decidedly varied amount of introspective preparation. Three have a fair musical training. One of these is especially delicate in rhythmical perception. One of the unmusical subjects, on the other hand, is especially imperfect as to all clearer rhythmical consciousness. Another is a Japanese. Questions have been asked for the subjective records as the state of the experiments seemed to indicate. Above all, I have wanted to know what it means to the subject to try to catch, to hold, to make an ideal for action, of this series of monotonous taps with their varying intervals. I have now about 200 of these subjective notes,

corresponding to about 1000 repetitions of the various rhythms.

Well, so far, I have been especially struck by the fact that the process of holding for imitation involves, according to the records, the most widely varying subjective processes, which do not seem to be constant, even for one subject, in any such way as you would expect. One catches the rhythm and prepares to repeat it by means of what appear in consciousness as the most heterogeneous materials. There is first, of course, the case where one tries, voluntarily or half involuntarily, devices which are either consciously abstract sorts of imitation, such as counting, or else involve the use of voluntary muscular movements, of hand or of foot, made in time to the rhythm while one hears it. But curiously enough, in many cases, and with some of the subjects, devices of this kind are felt as rather hindering than helping the imitation. Interesting also, with some subjects, is the lack of any preference for any particular set of these voluntary or semi-voluntary motor devices. But next, side by side with these voluntary processes, or instead of them, there appear unconsciously selected masses of varying organic feelings, which seem to be quite involuntary in their special origin and which are at least nearly always unexpected. These, when they come, keep some sort of time with the rhythm, and may help to apperceive it. They are described as inner beatings, 'in the head,' 'in the neck,' 'in one temple,' 'in the ball of my thumb,' as tinglings, throbbings, or what not. These vary most remarkably from experiment to experiment, appear to vary quite apart from one's expectation, to come and go as they choose. To these are joined on occasion all sorts of involuntary associations of a more or less symbolic sort—'ideas of urgency,' or of 'deliberation,' or of merriment, or of other such sorts familiar to all who note musical associations. Visual associations join themselves—a dark rhythm has been mentioned in one case. The visualization of the intervals as space intervals is not unknown. All these phenomena show so far a rather baffling variety, which forbids one easily to reduce them to the terms of habit. The whole process, at least in all its

earlier stages, show far less routine than I had expected. The report of definable 'waves of attention,' as such, has been rarer than I should have anticipated. Perhaps further introspection will distinguish these facts better. But, of course, when the rhythm is once well learned, all the foregoing processes may and sometimes do lapse into a mere sense, that 'I know all this.' Then, however, one still has a model general idea or ideal of the one rhythm, 'just like a sentence,' as the subjects are wont to say—a general idea of the one rhythm, which is still variable as to its tempo. By as elaborate devices for variation of the facts as I can devise, I am just now trying to run down what this general ideal of the variable unity of the one rhythm really is. But to speak of this would take me beyond my space. Nor have I as yet any report to make as to the time facts of the rhythm experiments.

I have meant to state a problem, viz., that as to the essential nature of the processes called imitative, and to report the mere fact of a research now under way. You may see how one of these reports suggests why the other is an indication of matters worthy of further study. Herewith my present purpose and your time are alike exhausted.

STUDIES FROM THE PRINCETON LABORATORY (I–V.)[1]

I. Memory for Square Size.

BY J. MARK BALDWIN AND W. J. SHAW.

The experiments of this study were performed at Toronto by Prof. Baldwin and Mr. W. J. Shaw, during the winter of 1892–3.[2] The object was to determine the accuracy of the memory for *size*, as affected by the lapse of time. A figure of two dimensions was selected for experiment because of the tendency to measure linear size in terms of well-known units of length. Circles tend to be measured by their radii, but in the case of the square, the impression is that of the area, and the natural memory-image is not so liable to be corrected by comparison with standards fixed in mind by repeated experience.

The experiments proceeded by three different methods: (1) *Selection from a Variety.* A single figure (the *normal*, 150 mm square) was drawn on a black-board and shown to a large college class; after a certain time a number of squares of various sizes were shown simultaneously, and the class was requested to designate the one that appeared to be the same size as the normal. The squares ranged from 130 to 210 mm by intervals of 20 mm, and the time intervals were 10, 20 and 40 minutes. The class consisted of about 225 persons, of whom some 50 were ladies. (2) *Identification.* Here the normal square was first shown, and afterwards one other square; the subjects were asked to say whether the latter appeared to be greater, equal to, or less than the normal. The time intervals were the same as before, and

[1] These studies were all concluded in the college year '93–'94.

[2] Reported in these words to the Amer. Psych. Ass., Dec., 1893, by H. C. Warren.

the second square was in every instance 20 mm greater than the normal.

Both series were treated by the Method of Right and Wrong Cases, and the two results showed remarkable agreement. The percentage of right cases is shown in Table I.

TABLE I.

	I. By Selection.	II. By Identification.
10 min. . .	64.1	87.6
20 " . .	59.3	82.7
40 " . .	36.4	58.5

Plotting the results (Fig. 1), we find the *memory curves*, as they may be called, practically parallel, but the degree of accuracy is much higher by the second method than by the first. In each there is a rapid falling off at first, then a period of gradual descent, and finally another rapid drop.

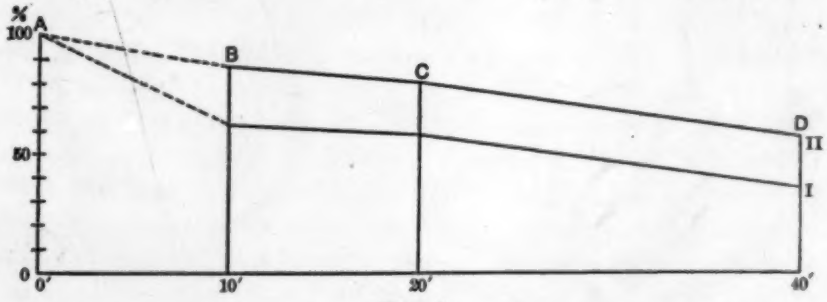


Fig. 1

The greater accuracy of the results in II is partly due to the manner of stating the question. Should the memory-image of the normal square either remain unaltered, or decrease in size, the subject would respond correctly that the second square was the greater, and he would respond incorrectly only if his memory-image had *increased* sensibly in size from its original. Whereas, in the series by Selection his responses would be classed as incorrect if his memory-

image had *either increased or decreased* sensibly. A further source of error in the series by Selection was the disturbance due to simultaneous contrast between the figures. Some special experiments were afterwards made to determine the effect of this contrast (see Study II, below).

In discussing the form of the two memory-curves so reached, it must first be observed that their real origin is not at A, but at a point, or points, near B. For the difference of 20 mm is very much greater than the least discernible difference between two squares observed in immediate succession; hence, even if a considerable interval should elapse before the second square is shown, *no incorrect judgment will be given.* The effect of this is to make the first falling off, when once it begins, even more rapid than indicated on the diagram, and possibly also to carry out the parallelism between the two curves still further. The reason for the sudden falling off may lie in the conditions of the experiments. The subjects began to take notes on a lecture immediately after the normal square was shown, and there was consequently a sudden withdrawal of attention from the memory-image, allowing it to fall into great indistinctness at once. After this first influence had taken effect, there was, it seems, but little change until the ordinary factors which tend to make the image more vague, began to take effect. The work of these factors, which one would scarcely expect to become apparent within 40 minutes, may have been hastened by the fatigue arising from steady application.[1]

(3) The third series proceeded by what was termed the *Method of Reproduction.* A normal square having been shown, as before, the subjects were asked, after the stated interval, to draw a square of the same size on paper. The normal in this case was 170 mm square. The reproductions were almost always too small, their average being 146.0 after 20 min. and 146.4 after 40 min. This result was rather unexpected, as the other series had indicated a tendency of the memory-image to increase in size beyond the original. It may be attributed to two factors: (1) The muscles of the

[1] The results were examined for a possible difference between the two sexes, but the variations were neither marked nor constant in direction.

hand were fatigued from continuous writing, and this tended to give the impression of a figure larger than that actually drawn. (2) The paper on which the drawing was made was not much larger than the actual size of the normal; any figure coming close to the edges would appear very large, since it occupied so large a portion of the field. Hence there was a tendency to draw the square too small. On this account it was decided to separate the results obtained by this method from the others, where the conditions were more nearly alike.

II. Further Experiments on Memory for Square Size.

BY H. C. WARREN AND W. J. SHAW.

The experiments were taken up at this point by Messrs. Warren and Shaw, at Princeton. A possible objection to the Selection Method lay, as has been said, in the disturbing influence of simultaneous contrast. To investigate this, the following experiment was performed: Ten squares, ranging between 100 and 190 mm, were drawn in promiscuous order on a large sheet of paper; on another sheet of the same size a single square was drawn as normal, and the two sheets were placed in different rooms. The subjects observed the normal first, and going at once to the other room designated the square which appeared equal to it. The normal used was 120 mm in one instance and 170 mm in another. In each case there was a marked *attraction towards the center of the series*, the average for the normal of 120 mm being 123.3, and for that of 170 mm, 165.

On this account it seemed desirable to supplement the Toronto experiments by others, and to employ a somewhat different method, using a series which combined the advantages of Selection and Identification. The object was to determine the threshold, *i. e.*, the (average) least perceptible difference from the normal after a given period of time. In each experiment the normal was first shown, and after the interval another square as near the threshold as the latter could be determined from the previous experiments; the experiments were continued until the threshold was found.

When the squares were shown in immediate succession (interval of *no* minutes = perception), the threshold was found to be 3 mm for squares of about 150 mm. When the interval was increased it was found to make an essential difference whether the second square was the larger or the smaller. For an interval of 10 minutes the threshold was 8 mm if the second was smaller, while it was but 5 mm if the second was larger; for 20 minutes it was somewhat less than 8 mm if the second was smaller, and less than zero (a minus quantity!) if the second was larger; that is, when two squares of the same size were shown, 20 minutes apart, *the second was pronounced the smaller* by over 50 per cent. of the subjects (actually, 63 per cent.)

That this result was not accidental (the conditions rendered any collusion impossible) was proved by the substantial agreement of all the experiments, pointing as they did without exception in the same direction. The entire series (marked *a* in Table II) was performed on the same subjects, a college class of about 50, Juniors and Seniors, on nine separate occasions, the 10-minute intervals being taken first. Besides this the table shows two experiments (marked *b*) on

TABLE II.

Interval and order.	Difference between I (normal) and II.						
	20 mm	12 mm	10 mm	8 mm	5 mm	3 mm	0 mm
ó . . . . . II < or > I	—	—	95 (c)	87 (c)	63 (c)	4 mm = 59 2 mm = 44 (c)	85 = (c)
1ó . . . . . II < I	—	—	—	—	50 (a)	—	—
1ó . . . . . II > I	87 (d)	—	70 (a)	53 (a)	—	—	—
2ó . . . . . II < I	—	—	75 (b)	94 (a)	75 (a)	65 (a)	63 < 24 = 13 > (a)
2ó . . . . . II > I	82 (d)	82 (a)	37 (b)	67 (a)	—	—	

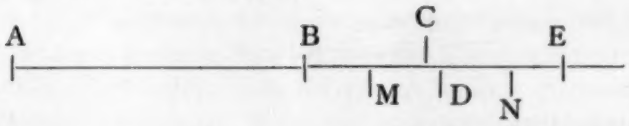
The figures denote percentage of right answers, except under 0 mm, where they denote the judgment (=, >, or <,) actually made. The normal was 150 mm square.

two other college classes of 50 and 65 respectively, where squares of 150 and 160 mm were used, with a 20-minute interval, the normal being smaller in the former case and larger in the latter. The lack of practice makes the threshold much greater in these instances than in the others, but they exhibit the same difference, depending on the order of sequence. The line of values marked *c* shows the experiments on squares immediately succeeding one another (0 minutes interval), taken with still another set of subjects, and the two values marked *d* are taken from the earlier experiments by Identification.

These results unite to show that besides the growth of inaccuracy, or indistinctness, in the memory-image, there is another factor at work, by which *the memory-image tends to grow larger* as the time interval increases. The table gives three cases which allow direct comparison between an increasing and a decreasing sequence: (1) With unpracticed observers (see *b*), 10 mm *increase* from the normal was noted by only 37% after 20 minutes, while the same amount of *decrease* was noted by 75%. (2) With practiced observers (*a*), 8 mm increase was noted by 67%, and the same decrease by 49%. (3) With the same observers as (2), the final test, after considerable practice, was with two equal squares, separated by 20 minutes interval; 63% pronounced the second square smaller, 24% equal, and 13% larger. Comparing this with the observations on the threshold for perception, we see that while half of the subjects can distinguish a difference in the latter case only when it amounts to 3 mm, in case of a 20-minute interval a majority actually think they perceive a difference *when none exists*, indicating plainly that their memory-image has grown by more than 3 mm, apart from any increase in the extent of the territory lying 'below the threshold.'

These results are not so satisfactory as the earlier series (see Table I) for determining the actual law of the threshold, on account of the increased degree of practice as the experiments proceeded. But they bring out clearly this fact of the growth, or *exaggeration*, of the memory-image. The following explanation, based on direct deductions from Weber's

Law, is suggested to account for this exaggeration. As given here, it assumes Weber's Law to hold rigidly; but even if we accept the latter only in the modified form that *the increments of sensation grow less rapidly than the increments of stimulus*, it will be seen to apply as a constant tendency, and will produce the result indicated, if the supposition on which it is based be admitted.



Let AC be the normal stimulus, and AB, AE the stimuli just perceptibly different from it. Then $\frac{AC}{AB} = \frac{AE}{AC} = r$, a constant for the entire series, according to Weber's Law; and $CE > BC$. Now the central stimulation of the memory-image may assume any magnitude $>AB$ and $<AE$, and any image within these bounds may be identified with the memory-image in respect to size. As there is no objective regulation of the stimulus, the actual representations will tend to distribute themselves, according to the theory of chances, evenly between B and E; but the images around B and E, and decreasingly towards C, will tend to be rejected as too small and too large, respectively. As the memory-image becomes indistinct, however, the imagination-images nearer C are less frequently rejected, and at length no images will be rejected between two given points, M and N. Now since the actual reproductions are distributed evenly between M and N, and none are rejected, the *average* of these will tend to assume the function of memory-image; that is, the point D, midway between M and N, will tend to become the basis of judgment, since AD is the average of the unchallenged images. But, since CN is always greater than MC, the point D will always lie beyond C; *i. e.*, $AD > AC$. Thus there will always be a tendency on the part of the memory-image to grow larger, as soon as there is any tendency on its part to become vague or indistinct; and the continuation of this process will be limited only by the limits of the vagueness of the memory-image, or by its relations to other objects or

memory-images which afford a means of comparison and regulation. In the instance at hand there are no such means of regulation within very wide limits, and the exaggerating process goes on without hindrance, so that in the end the point B is transferred to a position beyond C,—a result which, while unexpected and remarkable, is fully accounted for by the above theory.

The close of the college year prevented an extension of these experiments to intervals of 40 minutes with the same set of men.

A word or two may be in place here regarding the relation between single experiments on a number of subjects and a series of experiments on a single individual. In any experiments where a number of results are combined and their averages taken, what is sought is a *representative* value. By multiplying the trials, accidental influences are eliminated and we obtain a value representative of the given individual under the given conditions. If the individual represents some peculiar type, we should further compare his results with those obtained from individuals of other types. If, however, what we desire is the observation of an *average individual*, we must make sure that our subject is such, by comparing him with others. Rather than repeat the entire series on several individuals, we may save time and labor by performing a single experiment on a number together. There are then a number of precautions to be taken. (1) Each subject must understand perfectly the nature of the judgment to be made. (2) The judgments must be entirely independent. (3) The subjects must be representative—not drawn from some one peculiar class; and they must be governed by sensibly the same conditions. (4) Finally, care must be taken with the *objective* conditions of the experiment, so that no vitiating circumstances shall creep in.—In the present instance, every precaution was taken to fulfil the first two and the last of these requirements, and, a number of doubtful results having been rejected, the remainder fulfilled the conditions exactly, as far as a most careful scrutiny and attention on the part of the two observers could determine. Further, the subjects were acted upon by sen-

sibly the same conditions during the given interval. There is, of course, room for variety of opinion as to how far representative a college class is to be considered, and what allowances, if any, should be made for differences in previous occupation and differences in location with reference to the platform where the squares were shown. The writers are inclined to minimize these differences, and as to the former question, it is urged that a body of men like those under consideration are perfectly representative of the average educated male of about 21. We believe the results to be far more satisfactory than a quantity of experiments on merely one or two individuals, and think that this *cumulative* method, under which alone are possible certain experiments involving a great amount of time, may safely be used in connection with the more usual procedure.

III. The Effect of Size-contrast Upon Judgments of Position in the Retinal Field.

BY J. MARK BALDWIN.

I. *Problem, Apparatus, and Methods.* — The indication given in a preceding Study (I) that the arrangement of squares of various sizes in the visual field has an influence upon the identification of one of them as of a certain remembered size, suggested a farther research. It occurred to the writer that any influence of contiguous squares upon each other would be accurately measured by their joint influence upon the subject's estimate of some other distance on the retina. And such a distance as that lying between the squares lends itself directly to this purpose.

An arrangement was readily effected, whereby the ratio of the sides of two squares to each other was varied in a series of values, while the distance between the squares was kept constant. Any regular variations then in the judgment of this latter distance, such as that of its mid-point,—*i. e.*, the bisection of the distance between the squares,—would be due to the variations in the ratio of the square-sizes. Such a problem shows practical bearings also in all matters which

require estimates of balance, division, proportion in right lines between masses, objects, etc., in the field of vision: such matters as the hanging of pictures, all designing of cuts, vignettes, architectural plans, etc., which involve line values. Of course all variations from the correct location of a mid-point, or other critical point, lying between two masses of material, color, etc., should be allowed for in applying the formulæ of æsthetic effect.

A further complication also arises when movement enters into the case: the movement of the contrasted masses toward or from each other, of the eye from one to the other along the line of connection, or of the element of this line whose evolution describes the line.

Experimental Arrangements.—The following description (with Fig. 1) of my device for investigating the problem is given in some detail, since it meets the essential requirements of such experimentation and is so simple in principle that it may be adopted by others who desire to carry this kind of experimentation further.

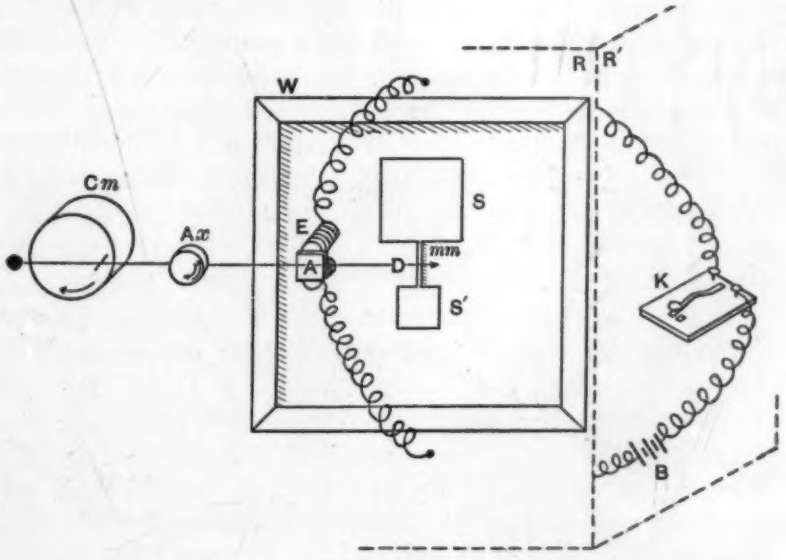


Fig. I.

The dark room (R) communicates with room I (R′) by a single window (W) which is completely filled with white

cardboard. In this cardboard two square holes are cut (S and S^1) whose sides are of determined ratio to each other, and whose distance from each other is measured by a slit (D) bearing a known ratio in length to the side of the larger square. On the wall beside the window (at Ax) is fixed the axis of movement of a long needle which is moved upon this axis by a pin carried round the face of the clock motor (Cm) of a Rothe polygraph. The movement of one end of the needle upward by the pin and downward by its own weight, is reversed by the other end of the needle, which so carries an arrow-head or pointed marker up and down the mm scale marked upon the slit D. The needle bears at A the armature of an electromagnet. The magnet (E) under the armature is fixed to the cardboard and its connections are carried into room R′ and terminate in a punch-key (K) on a table directly in front of the window W. The reagent sits at this key, closes the current when the needle reaches the mid-point of the slit D, the needle is arrested by the attraction of the magnet (E), and the reading is given on the scale mm. The apparatus works automatically, giving a series of experiments, with alternating up and down movement of the needle, until the motor runs down. A gas jet in room R is focussed through a large reading lens upon the scale mm, converting the small point of the needle seen by the reagent from the other room, into a moving bead of light. The back-ground of the squares and of the slit is the black of the dark-room wall, and the whole is seen by him upon the white surface of the cardboard.

For the horizontal arrangement of the squares, the whole apparatus is simply shifted 90°, bringing the axis of movement of the needle below the window.

With the arrangements thus described experiments were carried out on two persons; Sh., (W. J. Shaw) and T. (G. A. Tawney), with results as given in this report.[1] Both were practiced in psychological experimentation, but Sh. more than T.

[1] My thanks are due to these gentlemen, as also to two others who gave me some test series. For special reasons the conditions of reaction of the latter were not typical and so they were not continued.

In the case of each, the series of values of the ratio $\frac{S^1}{S}$ was $\frac{1}{2}$, $\frac{1}{4}$, $\frac{1}{8}$, $\frac{1}{16}$, which gives, when S has the constant value of 20 cm, the following series of values for S^1, *i. e.*, 10, 5, 2.50, 1.25 cm. A constant value for the distance between the squares was selected which seemed about as likely to occur in ordinary arrangements and experiments as any other, *i. e.*, $\frac{1}{2}$ S = 10 cm.

The experiments were performed in series of 20 to 25, called each a 'lot,' only one lot being taken at a sitting to avoid fatigue of the eyes. The time of day was kept constant, the subject was kept in entire ignorance of the object of the research and of the results he gave, and was asked after each series to give any impressions he might have of the accuracy of his results, and of the variations which he made, if any, in his method of identifying the mid-point. Careful record was kept of all these impressions, and they turned out to be valuable.

Methods of Identifying the Mid-point.—The two reagents began at the very beginning of the experiments to describe their procedure differently—a difference which was persisted in and became in the sequel a matter of fundamental importance. Sh. tended to fix his gaze upon the moving bead of light; followed it in its course, and stopped it when it reached the mid-point. This, it is evident, involves an element of eye-movement through a series of positions corresponding in extent directly to half the time D. This I shall call the 'approach method,' since the mid-point is selected only as it is approached by the light-bead.

T., on the other hand, tended to select the mid-point first; and endeavored to hold it fixed until the light-bead reached it, then fixing the bead by his reaction. This evidently gives a result largely independent of eye-movements on the line D, and this may accordingly be called the 'fixation' method. It will be seen below that very important consequences follow from this difference of method.

I. Approach Method. Vertical Arrangement. Results of Sh.—The results of 770 experiments with the vertical arrangement upon Sh., who used the 'approach' method, divided

into 5 series of 6 lots each, are shown in Table I. In the 'vertical arrangement' the larger square was above the smaller in all cases. The variable error is not given in any of the tables, since it fell below the limit of accuracy of the apparatus, *i. e.*, the diameter of the light-bead. The uniformity in direction of the constant error is shown in the small number of exceptions or minus judgments given in the column Excpts. in the table. The words 'down,' 'up,' 'both,' signify the direction of movement of the needle.

TABLE I.—Sh. App. Method. Ver. Arrgt.

No. Exps.	Ratio of Sides in cm.	Mean Var. in mm.			Excpts.
		Both Directions.	Down.	Up.	
155	20:10	2.35	2.7	2.	6
150	20:5	3.6	3.95	3.2	2
150	20:2.50	3.89	4.27	3.52	0
150	20:1.25	4.4	5.	3.8	0
165	20:0	2.8	2.93	2.66	1

The consideration of the figures given in this table enables us to formulate the following statements for the case in which the eye follows the stimulating bead to its point of arrest, up and down a vertical line:

1. There is a tendency to fix the mid-point too far away from the larger square (positive values of mean var.)

2. The direction of the tendency to error has practically no exceptions.

3. This tendency varies in some direct ratio with the ratio of the sides of the two squares to each other; *i. e.*, from .01215 of the side of the larger square when its ratio to the side of the smaller is 2:1, to .02 of the side of the larger when its ratio to the smaller is 16:1.

4. At the limiting value (0) of the side of the smaller square, the tendency to locate the mid-point too far away from the larger square is about the same as when the sides of the two squares are in the ratio 2 : 1.

5. The tendency to error is from 16 to 25 per cent. stronger when the stimulating object whose location is fixated is in movement in the same direction as the tendency of error (down), than when it is in movement in the opposite direction (up).

II. Results of Sh. Horizontal Arrangement.—Passing now to the horizontal arrangement, in which the details of apparatus remained the same as for the vertical, I may report as before for the two methods. The larger square was placed to the left, the smaller to the right, and the bead of light moved right and left over the slit between. The variations in the side of the smaller square gave as before the series of ratios to the side of the larger, $\frac{1}{2}$, $\frac{1}{4}$, $\frac{1}{8}$, $\frac{1}{16}$. The following table shows the results:

TABLE II.—Sh. App. Method. Hor. Arrgt.

No. Exps.	Ratio of Sides in cm.	Mean Var. in mm.			Excpts.
		Both Directions.	Right.	Left.	
100	20:5	.9	1.95	.33	20
50	20:2.5	1.67	2.5	.7	4
50	20:1.25	2.73	3.	2.46	2
50	20:0	2.1	2.	2.2	3

From the examination of Table II we gather the following results:

1. There is a practically uniform tendency of error away from the larger square.

2. This tendency varies in some direct ratio with the ratio of the sides of the two squares to each other.

3. The magnitude of the error is from .9 to 2.2 mm, *i. e.*, .005 to .01 of the side of the larger square.

4. At the limiting value (0) of the side of the small square the tendency is slightly less than when the ratio of the two sides is 16 : 1.

5. This tendency is about ½ greater when the movement of the stimulus fixated is in the direction of the error itself (right) than when it is in the opposite direction (left).

III. Fixation Method. Vertical Arrangement. Results of T.—The results of 683 experiments with the vertical arrangement upon T., who used the fixation method, divided into five series of six lots each, are as follows. See Table III.

TABLE III.—T. Fix. Method. Ver. Arrgt.

No. Exps.	Ratio of Sides in cm.	Mean Var. in mm.			Excpts.
		Both Directions.	Down.	Up.	
84	20:10.	2.96	1.99	3.85	4
150	20:5.	2.86	3.11	2.64	1
150	20:2.5	3.31	2.21	3.83	0
150	20:1.25	2.83	2.35	3.3	1
149	20:0	1.05	.8	1.35	21

Examination of this table enables us to make again the following statements for this subject with the method and arrangement described:

1. There is a tendency to error in the direction away from the larger square.

2. This tendency has so few exceptions that they are due probably to accidental causes.

3. The amount of this tendency is given in a number which fluctuates slightly about a value equal to .015 of the side of the larger square.

4. At the limiting value (o) of the side of the smaller square there is the same tendency to error, but it is less than ½ the error when the ratio is 1 : 2.

5. The tendency to error is about 50 per cent. greater when the stimulus for fixation is moving in the direction contrary to that of the variation itself than when it is moving in the same direction.

IV. Results of T. Horizontal Arrangement.—The experiments on T. with the horizontal arrangement, his method remaining as before that which I have called the 'fixation method,' gave the results shown in Table IV.

TABLE IV.—T. Fix. Method. Hor. Arrgt.

No. Exps.	Ratio of Sides in cm.	Mean Var. in mm.			Excpts.
		Both Directions.	Right.	Left.	
100	20:5	1.64	1.91	1.38	4
50	20:2.5	2.7	3.	2.5	0
50	20:1.25	3.25	3.65	2.9	0
25	20:0	2.6	1.53	3.9	0

From the examination of this table we may make the following statement of results for T.:

1. There is a uniform tendency to error in the direction away from the larger square.

2. This tendency is from 1.64 to 3.25 mm, *i. e.*, in this case .008 to .016 the side of the larger square.

3. This tendency varies in some direct ratio with the ratio of the sides of the two squares to each other.

4. At the limiting value (o) of the side of the smaller square the tendency to error is the same as when the ratio between the sides of the two squares is ⅛.

5. The tendency is about ⅓ greater when the stimulus fixated is moving in the direction of the tendency to error (right) than when it is moving in the opposite direction (up).

V. Rectification Method.—It is evident that a second series of indications may be obtained from the experiments given above in cases in which the reagent expresses his sense of the correctness or incorrectness of his result in each experiment. Both Sh. and T. were instructed to indicate after each experiment whether or not the bead gave a satisfactory result when stopped, and also in which direction the result should be rectified to give satisfaction. Records were kept of all such indications. Since it involved a secondary fixing of the mid-point, it approaches the 'fixation' method; but since it followed upon the earlier determination made when the needle was in motion, it involves influences akin to those of the 'approach' method; so it may be considered a combination of the earlier methods and a refinement upon both of them, for it requires a second act of judgment or criticism of the result already rendered in each trial. So let us call it the 'rectification' method.

It is further apparent that this rectification of the result of any given experiment may take one of four phases. It may be a judgment that the needle has gone too far, this we may call rectification by 'reversal;' or that it has not gone far enough, rectification by 'supplementing.' And each of these kinds of rectification will include again two instances. There will be reversals when the movement is in the direction of the prevailing error (*i. e.*, away from the larger square), and when the movement is contary to the direction of the prevailing error (*i. e.*, toward the greater square.) And the same two cases occur for the 'supplemental' rectifications.

The cases of rectification in the experiments on Sh. and T., both of whom were instructed to use the method, may be thrown into the following tables, in which the four kinds of rectification are distinguished.

Results for Sh. Rectification of Results Secured by Approach Method. Vertical Arrangement.—Giving the figures for Sh. in the vertical arrangement we have Table V.

TABLE V.—Sh. App. Method. Ver. Arrgt.

	No. Rects.	Ratio of Sides in cm.	Reversals.			Supplementals.		
			Movt. from S.	Movt. to S.	Total.	Movt. from S.	Movt. to S.	Total.
	15	20:10	1	9	10	3	2	5
	14	20:5	1	7	8	5	1	6
	17	20:2.5	1	9	10	6	1	7
	33	{20:1.25 / 20:0}	5	18	23	6	4	10
Totals . .	79		8	43	51	20	8	28

From this table we may conclude as follows:

1. Of the rectification of results secured by the approach method, the 'reversals' are nearly twice as frequent as the 'supplementals.'

2. The 'reversals' are 5 times as frequent when the bead moves against the tendency to error as when it moves in the same direction.

3. The 'supplementals' are $2\frac{1}{2}$ times as frequent when the bead moves in the direction of the error as when it moves in the contrary direction.

4. Rectifications take place in $\frac{1}{10}$ the entire number of experiments.

Horizontal Arrangement.—The rectifications of Sh. for the horizontal arrangement are shown in Table VI (first line).

TABLE VI.—Hor. Arrgt.

Subject.	No. Rects.	Method.	Ratio of Sides in cm.	Reversals.			Supplementals.		
				Movt. from S.	Movt. to S.	Total.	Movt. from S.	Movt. to S.	Total.
Sh.	38	App.	Whole series lumped.	8	24	32	3	3	6
T.	25	Fix.	"	12	7	19	1	5	6

It results from this table:

1. The 'reversals' number 5 times the 'supplementals' among the rectifications of data derived by the approach method.

2. The 'reversals' are 3 times as many when the bead moves in the direction contrary to the prevailing error (*i. e.*, toward the larger square), as when it moves in the opposite direction.

3. The supplementals are equally divided between the two cases of opposite movement of the bead.

4. The number of rectifications is about $\frac{1}{8}$ of the number of experiments.

Results for T. Rectifications of Results Secured by the Fixation Method. Vertical Arrangement.—The results of T. with the vertical arrangements appear in Table VII.

TABLE VII.—T. Fix. Method. Ver. Arrgt.

	No. Rects.	Ratio of Sides in cm.	Reversals.			Supplementals.		
			Movt. from S.	Movt. to S.	Total.	Movt. from S.	Movt. to S.	Total.
	10	20:10			0	7	3	10
	19	20:5	12	5	17	1	1	2
	19	20:2.5	3	1	4	6	9	15
	31	20:1.25 / 20.0	7	9	16	4	11	15
Total . . .	79		22	15	37	18	24	42

Table VII shows the following:

1. Rectifications by 'supplementing' are $\frac{1}{7}$ more frequent than those by 'reversal' when the results are secured by the fixation method.

2. The 'reversals' are $\frac{1}{2}$ more frequent when the bead moves in the direction of error than when it moves in the contrary direction.

3. The 'supplementals' are $\frac{1}{3}$ more frequent when the bead moves in the direction contrary to that of the prevailing error than when it moves in the same direction as the error.

4. The entire number of rectifications is $\frac{1}{9}$ of the entire number of experiments.

Horizontal Arrangement.—The rectifications of T. for the horizontal arrangement are given in Table VII (second line).

1. *Results.*—The 'reversals' are three times the 'supplementals' in the fixation method, horizontal arrangement.

2. The reversals are $\frac{1}{2}$ more when the bead moves in the direction of error than when it moves in the opposite direction.

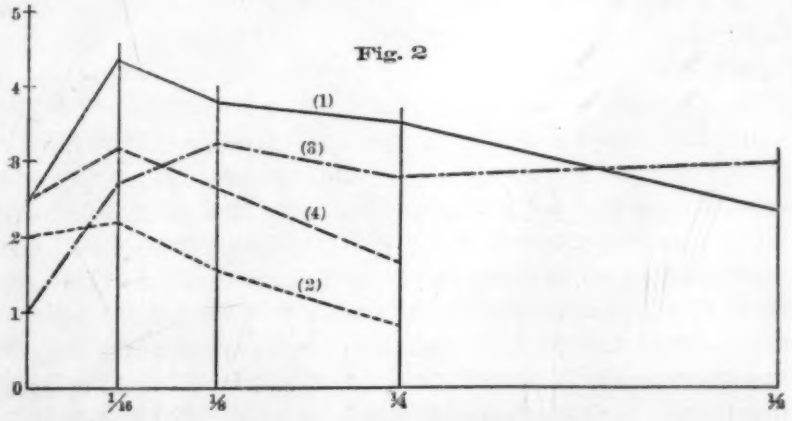
3. The 'supplementals' are five times more when the bead moves contrary to the direction of error than when it moves in the same direction. This result, however, is based on too small a number of cases to be taken as a numerical ratio.

4. The number of rectifications is $\frac{1}{9}$ of the whole number of experiments.

VI. General Interpretation of Results.—We are now able to gather up the results shown in the earlier tables in some more comprehensive statements, based upon the whole number of experiments taken together.

I. Considering the results for the direction and amount

Fig. 2



of error without regarding the influence of the direction of movement of the light-bead, we may plot curves showing

the tendency and amount of error for each of the two arrangements by each of the two methods. In Fig. 2 the horizontal ordinate represents the constant series of ratios of the square-sides to each other; the vertical ordinate, the size of the error and its duration (above the abscissa denoting error away from the larger square). Curves (1) and (2) give the results by the approach method, vertical and horizontal arrangements respectively; curves (3) and (4) the results by the fixation method, vertical and horizontal respectively. The location of the various points of the curves is determined in each instance by the figures given in the appropriate table above. The curves are numbered to correspond with the respective tables.

Inspection of the four curves gives certain general results which unite and summarize the results already shown from the separate tables above.

1. The four curves (representing 1,928 experiments) agree in establishing a tendency to error away from the larger square of from 1 to 4.5 mm when the side of the larger square is 20 cm.

2. The close parallelism of three of the curves in their common direction, and the general parallelism of all the four, establishes the fact that the tendency to error increases with the relative increase of the side of the larger square.

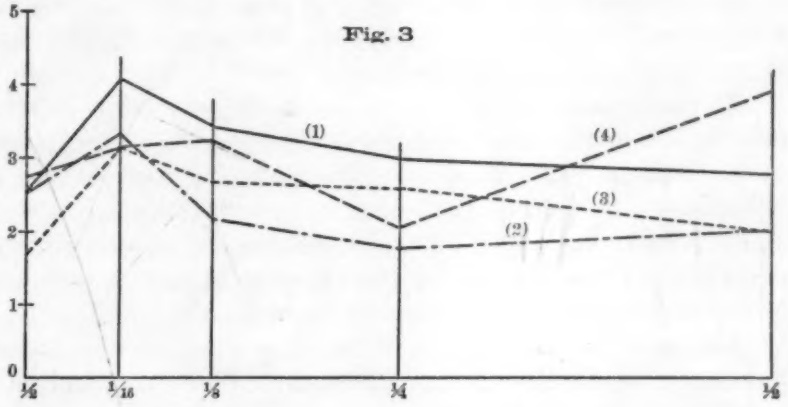
3. The position of curves (1) and (3), considered in relation to the position of curves (2) and (4), shows that the tendency to error, when the squares are arranged vertically, is about twice as great as when they are arranged horizontally.

4. Comparison of curves (1) and (2) with curves (3) and (4) shows that the method of fixation gives more uniform results than the method of approach; and also that the difference in result between the vertical and horizontal arrangements is less when the fixation method is used. It follows from this that eye-movements over a line hinder the correct estimate of the parts of that line, and that this influence of eye-movement is greater for vertical than for horizontal directions.

II. Considering the results with regard to the direction of movement of the light-bead by both methods and in both

arrangements, we may plot the curves of Fig. 3, in which the ordinates remain as in Fig. 2, the points on curves (1) and (2) give the amount of error for the several contrast ratios for the case of movements of the bead away from and toward the larger square respectively by the approach method, and the points on curves (3) and (4) give the amount of error for the same two cases respectively, by the fixation method. These amounts are reached by combining the figures for 'down' and 'right' movements in the tables of vertical and horizontal arrangement of the approach method, for each contrast ratio, and combining similarly the 'up' and 'left' results of the corresponding tables of the fixation method.

Fig. 3



Inspection of these four curves (again representing the entire 1,928 experiments) leads us to certain conclusions.

1. Comparison of curves (1) and (3) with curves (2) and (4) shows that the error is greater when the bead is moving in the direction of the error.

2. This is especially the case when the approach method is adopted, the error then being twice as great when the movement is in the direction of the normal error as when it is in the contrary direction [comparison of curves (1) and (2)].

3. It follows that the influence already found to be due to eye-movements varies according to the particular direction of the movement along the line explored. If the eye-movement is toward the larger of the areas contrasted, it tends to cor-

rect the normal error of judgment in the estimation of the time which connects the two areas. If the movement is, on the contrary, away from the greater area it exaggerates the normal error of judgment.

III. The details of the instances of 'rectification' given above serve to confirm these general conclusions, both as to the normal error itself and as to the influence of eye-movements upon it. By the approach method the rectifications by reversal are two to five times more frequent than those by supplementing. This shows that the rectifications in this instance are really corrections of the influences now found to be due to eye-movements. Further, reversals are three to four times as frequent when the bead moves against the tendency to error as when it moves in the direction of this tendency. This shows that these corrections are much more likely in direction opposite to that in which we now find the real contrast error to occur. When moving in the direction of the contrast error the eye-movement influence gets support from that error, and so fails of detection, and even secures supplementing in this direction more frequently than the movement in the opposite direction does. This is an indirect determination of the true direction of the contrast error in agreement with the direct experimental result.

The rectifications in the fixation method, on the other hand, are equally divided between the 'reversals' and the 'supplementals,' showing that the influence of eye-movement is largely eliminated by this method. And further, the distribution of both supplementals and reversals between the two cases of movement, in one direction or the other, is now directly reversed, *i. e.*, the reversals are more frequent when the bead moves in the direction of error, and the supplementals when it moves contrary to this direction, a result which seems to show that in this case the tendency to error from contrast is in conflict with the normal influence of eye-movements, and the correction is made to increase the latter in one direction, and to diminish the former (or their sum) in the other direction.

The entire number of rectifications of all kinds (about $\frac{1}{8}$ of the whole number of experiments) may be taken as a sort

of quantitative indication of the function of second-judgment, or deliberation, upon sensory determinations of such a complex character as those involved in these experiments. It is interesting to note that this second judgment, however, does not tend in the general result to correct the error of first judgment; for there are about ½ more cases of rectifications by displacement toward S^1 (the direction of the error) than toward S. The only case in which the correction does work to give greater accuracy to the result is that of the use of the fixation method, where both the original and second judgments are comparatively free from eye-movements and their after effects.

Finally, the great uniformity of the error of judgment is seen in the small number of cases (69 in the entire series of 1,928 experiments) in which the mid-point was located in the direction opposite to the prevailing error (that is, located too far toward the large square). And even this number represents too high a figure, since the sum of the variations of this kind in all but two series gave only 28 cases (*i. e.*, in 1,679 experiments); the two giving the very abnormally large figures 20 in 100 experiments (app. method, horiz. arrangement) and 21 in 149 experiments (fix. method, vert. arrangement) being evidently affected by some temporary influences.

A series of experiments has already been begun with a stationary stimulus (thus ruling out the influence of eye-movements); and I hope also to complicate the case with variations planned to introduce æsthetic elements into the problem.

IV. Types of Reaction.

BY J. MARK BALDWIN (with the assistance of W. J. Shaw.)

The experiments reported in this article were carried out in the Toronto Laboratory in 1892–93. Three questions were set for research, all of them bearing on the question of the degree of relativity of reaction times: either the difference of a single individual's times, according as there

were subjective (attention) or objective (qualitative stimulus) changes in the conditions of his reaction; or the differences of reaction times for different individuals under identical conditions. To secure results comparable in the respects in which comparisons were desired, certain precautions were made, as follows: (1) each reagent reacted at the same hour from day to day, and at the same hour with each other reagent whose reaction was to be compared with his; (2) the order of change in the conditions of reaction (as sensory-motor, light-dark, visual-kinæsthetic, etc.) was kept in the main the same for the different reagents.

The Hipp and D'Arsonval chronoscopes were used, both controlled by the records of a König tuning-fork recording on the drum of the Marey motor. The 'light' reactions were taken in a room of good south morning exposure, and those in the dark, in a dark closet of the same room. The stimulus was in all cases an auditory one—a sharp metallic click—and the reacting movement was a pressure downward of the right forefinger (in the case of the D'Arsonval instrument, a pinch of that finger and the thumb). The reagents were, besides the writers (B. and S.), Mr. Faircloth (F.), a student who had had only the experience gained from the practical work in this subject of the course in Experimental Psychology. His reactions were ready and unconfused, and from all appearances he was a normal and more than usually suitable man for such work. The fourth, Mr. Crawford (C.), is an honor student in this subject in Princeton. His reactions were taken in the course of another investigation, and being so few in number, they are included only because they give a certain case of a capable reagent whose sensory is shorter than his motor reaction. We hope to test him further.

I. *Variations in the Results.*—Table I. shows the relative reliability of the two instruments in these experiments.

TABLE I.—Clock-corrections.

Instrument.	Const. Error.	Var. Error.
D'Ars.,	.19	.06228
Hipp,	.019	.015616

All the results secured by each instrument are corrected, by the constant error of that instrument, before being used either for comparison among themselves or for compounding with the results of the other instrument, in the tables which follow. The smaller variable error of the Hipp chronoscope makes the results of that instrument much more reliable in the matter of absolute time-measurement. But in the conclusions drawn below, only those results are used in which the quanity sought is a relative one, and in which the two clocks confirmed each other in giving ratios of difference of the two quantities compared, both of which are in the same sense, and each of which is larger than the largest possible ratio of difference arising from the variable error of the clock to which it belongs.

The mean variations are not given in the tables which follow, because they are too complex to be of any value. These variations were different for the two clocks, as we would expect from the variable errors of the instruments themselves; they also varied with the disposition of the subject in the various groups of results which are treated together.[1] The different mean variations for the different lots of experiments varied from 10σ to 20σ. For this reason no deductions are attempted except those evident on the surface of the results themselves.

II. *Results: Sensory and Motor Reactions.*—Table II. gives the results of experiments on four persons designed to test the current distinction between 'sensory' and motor ('muscular') reactions.

[1] Finer distinctions were aimed at in some of the series, such as placing the sound stimulus on one side only, or in the median plane below the head, etc., as well as arranging for the difference between light and dark environment.

TABLE II.—Types of Reaction.

Subject.	No. Exps.	Sensory.		Kin. Motor.		Vis. Motor.		Av. Motor.	
		No.	Time in σ.	No.	Time in σ.	No.	Time in σ.	No.	Time.
B.	2490	1043	178	966	149	481	171		160
S.	2572	1017	235	995	184	560	195		187
F.	820	290	164	260	202.3	270	205.5		204
C.	212	84	132					128	157

It follows from Table II.: (1) that the current distinction between sensory and motor reactions does not hold in the sense that the motor reaction is always shorter than the sensory, for in the case of F. the motor reaction is 40 σ longer, *i. e.*, $\frac{1}{5}$ of this subject's average sensory reaction time. (2) As between B. and S., in the case of each of whom the motor-time is shorter, there is a great difference in the relative length of the sensory to the motor. In B. the sensory time is only 18 σ, or about $\frac{1}{9}$ longer than the motor, while in the case of S. the sensory is 48 σ longer, or about $\frac{1}{4}$; and this despite the close agreement of the two subjects in their absolute motor-time. We would seem to have, therefore, in these three observers three cases shown, two giving very pronounced results; one a longer motor time by $\frac{1}{5}$, and the other a longer sensor by $\frac{1}{3}$. The third subject, B., seems to fall between these extremes, giving a difference in favor of the motor reaction, it is true, but a much smaller difference.

The tables also give us reason for accepting the truth of the distinction between two kinds of motor reaction. In both B. and S., whose motor reactions are shorter than the sensory, we find a difference in the length of the motor reaction according as the attention is given to the movement by thought of the hand, the eyes being blindfolded; or as the attention is fixed upon the hand, which is seen. The former I have called the *kinæsthetic motor* reaction, the latter

the *visual motor*. In B. the visual motor is 22σ, or about $\frac{1}{7}$ longer than the 'kinæsthetic'—that is, it is practically equal to this subject's sensory time; while in S. the kinæsthetic motor is 11σ shorter than the 'visual.' With F., on the contrary, there is no distinction between the two kinds, any possible trace of it seeming to be lost in the excessive preponderance, in facility, of the sensory kind of reaction.

The table as a whole, then, supports the views: (1) when the motor reaction is short in relation to the sensory (case of S.), then this motor reaction is purest, freest from sensory influences, such as sight, etc.; (2) when the motor reaction is not pure, then it is retarded by such influences as sight (case of B.); (3) where the motor reaction is relatively difficult and delayed, as compared with the sensory (case of F.), there this prime difference renders all kinds of motor reactions equally lengthy and hesitating. B. seems to stand midway between the two others in this respect.

As I said some time ago, in making a first report upon the outcome of some of these experiments:[1] "In subjects of the motor type the 'kinæsthetic motor' is shorter, the 'visual motor' time approximating the sensory reaction time."

III. *Light and Dark Reactions to Sound.*—The foregoing deductions concerning the difference between B. and S., as respects motor and sensory reactions, and also as respects the distinction between visual and kinæsthetic motor reactions, are confirmed by results of a research on the same two subjects, in which the attempt was made to investigate the influence of vision. Each reagent gave a series of reactions in the light of an ordinary laboratory room, and then repeated the series under the same general conditions, but in a dark chamber. In this case, in order to make the results of the two series comparable, the kinæsthetic form of motor reaction was necessary in the series taken in the light, since only that kind of motor reaction was possible in the dark. The results are given in Table III.

[1] New York *Medical Record*, April 15, 1893, p. 455 f.

TABLE III.—Reactions in Light and Dark.

SUBJECT.	LIGHT.				DARK.			
	Sensory.		Motor.		Sensory.		Motor.	
	No.	Av. in σ.	No.	Av. in σ.	No.	Av. in σ.	No.	Av. in σ.
B.	541	176	979	164	502	184	468	138
S.	537	237	1190	158	480	219	469	179

On examination the data of this table, compared with those of the preceding table, may be stated as follows: We find for B. that the sensory reaction is practically the same, whether he react in the dark or in the light (the latter is less by 8σ, which is insignificant in view of the variable error). This shows this subject's independence of vision in the sensory reaction to auditory stimulations, and is in agreement with the results of Table II. (in which there is a similar difference between the sensory and visual motor, the former being longer by 7σ). S., on the other hand, shows a shortening of the sensory reaction when in the dark by 18σ, but a lengthening of the motor reaction when in the dark by 21σ, or about $\frac{1}{7}$. The latter result shows this subject's dependence upon vision only in the motor kind of reaction.[1]

IV. *Interpretation.*—Admitting that these results indicate clearly the existence of persons whose sensory reactions to sound are shorter than their motor reactions, and that there is in some individuals a difference between the length of the motor reaction, according as it is made in the light or in the dark, we may make some general remarks on the theory of these differences. These results should be compared with earlier ones, a matter which is made easier by reference to the concise summing up of the literature of the subject by

[1] The 'dark-reaction' was not secured from F., the 'sensory' subject; but we hope to report further results obtained from C., the similar case now found in Princeton.

Titchener in *Mind*.[1] We find cases of relatively shorter sensory times similar to mine reported (for electrical stimulus) by Cattell,[2] and (for sound stimulus) by Flournoy[3]. We may accordingly say that such individual differences are clearly established, and must hereafter be acknowledged and accounted for in any adequate theory of reaction.

The attempt of Wundt, Külpe and others to rule these results out, on the ground of incompetency in the reagents, is in my opinion a flagrant *argumentum in circulo*. Their contention is that a certain mental *Anlage* or aptitude is necessary in order to experimentation on reaction-times. And when we ask what the *Anlage* is, we are told that the only indication of it is the ability of the reagent to turn out reactions which give the distinction between motor and sensory time, which Wundt and his followers consider the proper one. In other words, only certain cases prove their result, and these cases are selected because they prove that result. It is easy to see that this manner of procedure is subversive both of scientific method and of safely-acquired results in individual psychology. For the question comes: what of these very differences of individual *Anlage?* How did they arise; what do they mean; why do they give different reaction-time results? To neglect these questions, and rule out all *Anlagen* but one, is to get the psychology of some individuals and force it upon others, and thus to make the reaction-method of investigation simply the handmaid to dogma.

The attempts to explain the relative shortness of the 'muscular' reaction, also, by those who hold its shortness to be a universal fact, have been unfortunate. It has been

[1] Jan., 1895, p. 74.

[2] *Philos. Studien*, VIII., 403.

[3] *Arch. des Sci. Phy. et Nat.*, XXVII., p. 575, and XXVIII., p. 319. Titchener, in his summing up, does not cite the cases of Flournoy nor the earlier report of one of my present cases (F.) in the *Medical Record*, Apl. 15, 1893, although they tell directly against his own views. My earliest case was noted by me in the autumn of 1892, and the note in the *Medical Record* was written in December, 1892, before I saw either Cattell's or Flournoy's articles. The sentences quoted from my *Senses and Intellect* by Titchener in *Mind*, *loc. cit.*, were based upon my own reaction-times, taken earlier when I had no reason to doubt the universality of the experience, as claimed by Lange and Wundt. Titchener is accordingly wrong in citing me as favoring that position.

held that the muscular reaction is shorter because it is semi-automatic; the thought of a movement, *i. e.*, attention to it, being already the beginning of the innervations necessary to its production. This is very true as a principle, I think; but it is just the application of this principle which makes it necessary on the part of some to restrict reaction work to people of a special aptitude. For in all those cases, either of particular reactions in one individual or of all reactions in other individuals, in which the movement is not so clearly picturable as to be firmly and steadily held in the attention, to these cases the principle does not apply. On the contrary, to all cases where it is difficult to get the attention fixed upon a *motor* representative of the movement, a very different principle applies, as Prof. Cattell has said. The very attempt to picture a movement *as a movement*—by putting the attention on its *motor* aspect in consciousness—embarrasses, confuses and delays the execution of that movement in these cases. If a marksman attend to his finger on the trigger he misses the target; if a ball-player attend to his hands he 'muffs' the ball; if a musician think of each finger-movement he breaks down. The musician in the laboratory is usually, indeed, a glaring instance of unsuitable *Anlage!*

So it is evident that these two principles need reconciling in their application to reaction-times, the principles, *i. e.*, (1), that *the thought of a movement already begins it, facilitates it, quickens it;* and (2) *that attention to a practised movement, in many instances, embarrasses it, hinders it, lengthens it.*

Now the practical reconciliation of just these two principles has been made in another great department of fact, and the actual plotting done of the cerebral arrangements which underlie them—a solution which has such evident application here that I wonder at its tardy appreciation. I refer to the work in the pathology of aphasia, and the general theory of mental 'types' which now goes for a safe discovery in the discussions of 'internal speech,' 'sensory *vs.* motor defects' of speech, etc. I published early in 1893[1] an hy-

[1] See the *Medical Record* (N. Y.), *loc. cit.*

pothesis to account for the variations in this matter of reaction-time differences, in these words:

"I have endeavored incidentally, in an article now in print for the July issue of the *Philosophical Review*,[1] to account for the conflicting results of experiment in this field, by borrowing from the medical psychologists the results of their brilliant analysis of the speech function, on the basis of its pathology. The recognition of the great forms of aphasia—*i. e.*, sensory and motor—and the corresponding recognition of the existence of visual, auditory, and motor speech types, gives a strong presumption that the distinction between sensory and motor in the voluntary movements of speech and writing applies as well to voluntary movements of all kinds; that is, to all movements which have been learned by attention and effort. This means that a man is an 'auditive,' or a 'visual,' or a 'motor' in his voluntary movements generally. His attention is trained by habit, education, etc., more upon one class of images than upon others, his mind fills up more easily with images of this class, and his mental processes and voluntary reactions proceed by preference along these channels of easiest function.

"If this be true, it is evident that a man's reaction-time will show the influence of his memory type. The motor-reaction we should expect to be most abbreviated in the man of the motor type; and less abbreviated, or not so at all, in the 'visual' or 'auditory' man. And experimental results must perforce show extraordinary variations as long as these typical varieties are not taken account of. We are accordingly, I think, a long way off from any such exact statement of absolute difference between sensory and motor reaction-time as Wundt makes in his last edition."[2]

It was a sense of the great naturalness and probability of this hypothesis that led me early in the fall of 1892 to institute the experiments on 'visual' and 'kinæsthetic' motor reactions whose results are given above in this paper.[3]

The secure establishing of cases which show sensory reactions shorter than motor (*i. e.*, the cases now reported by Cattell, Flournoy and myself), together with the probable

[1] Article entitled "Internal Speech and Song," *Phil. Rev.*, July, 1893.

[2] *Physiologische Psychologie*, 3d ed., II., p. 261 ff.

[3] At the Philadelphia meeting of the American Psychological Association, on Dec. 28, 1892, I proposed the hypothesis informally to several American psychologists. Dr. Lightner Witmer will remember a conversation in which the point was remarked upon. I venture to make these personal explanations since a somewhat similar explanation of his cases was advanced by Prof. Flournoy, of Geneva, in the articles cited above. I was not acquainted with Prof. Flournoy's views until, a year later at the New York meeting of the Association, Prof. Cattell brought them to my attention, as given in abstracts in the *Revue Philosophique* and the *Zeit. für Psych.* I return to Flournoy's position later on in this paper.

distinction between the 'visual' and 'kinæsthetic' forms of motor time, make it advisable that this hypothesis should be put in clearer evidence. I shall therefore proceed to state the case for it briefly on the basis of the facts as they are now known.

The doctrine of 'types' rests upon certain facts which may be briefly summed up. A voluntary motor performance—say speech—depends in each particular exercise of it, upon the possibility of getting clearly in mind (*intérieur, innerlich*) some mental picture, image, presentation, which has come to stand for or represent the particular movements involved. This mental 'cue' or representative may belong to either of two great classes: it may be a 'sensory' cue or a 'motor' cue. People are of the sensory type or of the motor type for speech according as their cue in speech is sensory or motor; that is, according as in speaking they think of the sounds of the words as heard, the look of the words as written, etc.—the cues furnished by the special senses associated habitually with speech—this on the one hand; or according as, on the other hand, they think of or have in mind the movements of the vocal organs, lips, tongue, etc., involved in speech. In the 'motor' people there are incipient movements in mind; in the 'sensory' people there are special sense images in mind. All this is now so clear from the pathological cases examined that the theory of localization of brain areas and their connections is applied to the successful exploration of damages of the brain when aphasic symptoms furnish the main diagnostic resource.

Now, let us see how in these cases of aphasia the two principles spoken of are applied. Suppose we agree with the neurologists in saying that the function of the 'cue'—the mental image, be it either motor or sensory, which when thought of enables a man to speak—is to release energy from its own brain-seat, along association fibres or pathways, to the motor-seat which sends its discharges out to bring about the movement. Then the difference between sensory and motor people is simply that different centres—different 'cue'-seats—have these connections with the motor speech centres best or better developed. A man who speaks best

when he thinks of the sounds of the words has his best 'cue'-seat in the auditory centre; and his best pathway to the speech motor-centre goes out from this 'cue'-seat. For the man who speaks best when he thinks of the utterance of words, the same may be said of the muscle-sense seat.

So it is evident—apart quite from the question as to how one or other state of things comes to be as it is in any one case—that with one man attention is directed to the *movement* for the best results, with another to the sensation or special memory image in best association with the movement. With the former *the thought of the movement begins the movement.* But with the other, if the best doing of the movement comes from thinking of a sensation or special image, *then the movement will be relatively deranged, embarrassed, when the attention is drawn from this sensation* and forced to fix itself upon the movement itself. These, then, are the two principles we desiderate, and they are both natural parts of the 'type' theory.

So why not generalize this? Speech cannot be considered an exceptional function in its rise and mechanism. Other complex motor functions show the same kinds or types of execution: handwriting, music performing, etc.[1] The hand has, next to the tongue possibly, the most delicate, varied and differentiated functions to perform; and the laws of association by which sensory cues, checks, controls, are affixed to hand actions and combinations, must be the same as those involved in speech. So in simple hand movements people must show the sensory and motor types. This is my hypothesis.

The man, therefore, who gives relatively shorter motor reactions is a 'motor' in his type; with him the thought of movement is the most facile beginning of the movement, just because it is *really the movement*, and nothing else, that he thinks of. That is his *Anlage*. But the man who gives relatively shorter sensory (auditory, visual) reactions, is a 'sensory' in his type: with him the attempt to think of the

[1] See my *Mental Development: Methods and Processes*, pp. 91 ff., and 438 ff. In Chap. XIV. of that work, on 'The Mechanism of Revival,' I have endeavored to put in evidence the general principles which underlie the type theory.

movement *as a movement* interferes with the prompt and exact execution of it, just because he is not accustomed to execute his movements in that way. That is his *Anlage*. But, of course, the two sorts of people have equal claim to recognition in science. Suppose a dead aphasic brought for autopsy to a surgeon, who inquires into the life-history of the man, and finding that he was of the sensory type, then declares that the body is not fit for a scientific autopsy, because the man did not have the proper type of aphasia! As a matter of fact, so near are the disciples of Wundt to the explanation by types that it is only necessary to translate their word *Anlage* by 'type,' and then apply the connotations of that term in the examination of refractory cases, to bring them into line. I may accordingly sum up in the words of my earlier article (*Philos. Rev.*, II, 395):

> "We have in this fact of types the explanation of the contradictory results reached by different investigators in the matter of motor reactions. Some find motor reactions shorter, as I have said above; others do not. The reason is, probably, that in some subjects the 'sensory' type is so pronounced that the attention cannot be held on the muscular reaction without giving confusion and an abortive result. On the other hand, some persons are so clearly 'motor' in ordinary life, that sensory reaction is in like manner artificial, and its time correspondingly long. And yet again others may be neutral as regards sensor or motor preferences. If this be true, another element of 'abounding uncertainty' is introduced into all the results of experiments so far performed in this field, as reflection on the matter will show."

One or two further points, however, may be made which give the correct interpretation more importance than the simple facts in themselves really have. In the first place, an additional tendency seems to show itself when movements become very habitual—a tendency recognized in all discussions of the principle of habit. Habitual performances tend to become independent of consciousness, attention, thought, altogether. This tendency should make itself evident in reaction-time work, and reagents of great practise should show, (1) diminishing time in all reactions, and (2) diminishing difference between the two kinds of times, sensory and muscular. Further, the same tendency should show itself in a diminishing difference between individuals

of different types as they both get more practise. All these results are, I think, clearly shown in those of the earlier researches in which the amount of practise is reported.[1]

And, again, finer distinctions of type follow from the general theory: such distinctions as those clearly established for speech. The 'visual,' 'auditory,' and possibly (as in the blind) 'touch' types of speech are all included under the head of sensory. As I have said, the speech case *is* a case of finer reaction-time distinctions. And the hand, as used in most reaction experiments, ought to show to a greater or less degree similar distinctions.[2] The cases so far discovered of relatively shorter sensory reactions seem to be, as far as reported, auditory (musicians) and visual (Flournoy's). To determine between 'visual' and 'auditory' times for any individual, of course the same set of reaction experiments should be made with the two classes of stimulations, each being compared with the muscular reactions to the same stimulus respectively.

The general result follows (if this hypothesis get acceptance) that the reaction-time experiment becomes of use mainly as a *method*. Distinctions supposed to be established once for all by various researches must be considered as largely individual results, inasmuch as the authors have not reported on the type of the reagent. But for that very reason these results may have great value, as themselves indicating in each case this very thing, the type of one single reagent, and in so far some of the general characteristics of that type. What we now desiderate in a great many departments, as, for example, in the treatment of school children, and in the diagnosis of complex mental troubles, is just some method of discovering the type of the individual

[1] Consequently it does not do to say, with Wundt and Külpe, that the 'muscular' reaction is more automatic. Of course it is so in those who give a shorter motor reaction—that is sufficient proof of it. But that the sensory time is shorter in others is sufficient proof, also, that in their case the sensory reaction is more automatic. Külpe's two-arm reaction experiment is subject to this criticism, among others (see Wundt, *loc. cit.*, p. 325; Külpe's *Grundriss*, pp. 422 f.).

[2] A possible instance of such variation is seen in the case of Donders, which Wundt has difficulty with (*Phys. Psych.*, III^e., II., p. 268). Say the reagent was 'visual' in his type, and we have reason for his shorter reactionto light than to sound, while he still falls under the sensory type in general.

in hand. If reactions vary in certain great ways, according to the types which they illustrate, then in reaction experimentation we have a great objective method of study. But before the method can be called in any way complete, there should be a detailed re-investigation of the whole question, with a view to the great distinctions of mental type already made out by the pathologists.[1]

A word should be added concerning the position of Professor Flournoy. The hypothesis which I have advanced has been attributed also to Flournoy, his name being connected with mine as independent advocates of it (by Cattell, Titchener, etc.). I think this is a mistake, at least so far as the publications of Professor Flournoy are taken as evidence. His case, cited as of the 'type visuel,' seems to imply the existence of other types it is true; and at the close of his second article he raises the question, "si la façon de reagir observée chez M. Y., n'est qu'une singularité individuelle, ou si elle est un fait général et constant dans le type visuel d'imagination." But what he means in the context by 'type visuel' is not what is meant by that phrase in the generalized usage of the pathologists. His case is 'visual' in the sense that the man thinks of movements by a visual picture of his arm, rather than by muscle-sense images (just what I have distinguished above as 'visual motor' in distinction from 'kinæsthetic motor;' and the case is a fine confirmation of the conclusions given above under that head). But it does not follow that the man is a 'visual' in the broader sense. He might just as likely be an 'auditive.' The most that can be said of Flournoy's case, on the general doctrine of types—other evidence aside—is that he is 'sensory,' and on my theory his shorter sensory reaction-time proves it. But Flournoy makes no such general application of the theory of types. Indeed, in asking the question which I have quoted from him (*i. e.*, whether all visuals would react as this man did), he shows that he does not mean

[1] I have earlier indicated (*Med. Record, loc. cit.*, and *Philos. Rev., loc. cit.*), the possible use of this method by brain surgeons, an idea which Wallaschek comments on with approval. Certain general indications from reaction-times are already recognized by physicians, especially in investigating various anæsthesias.

to bring reactions generally under the type theory. For the real 'visual' might give a shorter 'visual motor' than 'sensory' time—*i. e.*, when the stimulus reacted to is other than visual (say auditory); since then the *visualizing habit* would throw its influence on the side of the motor reaction.

In the matter of the distinction between 'visual motor' and 'kinæsthetic motor' reactions, however, Flournoy's case clearly anticipated mine in print.[1]

V. Sensations of Rotation.

BY H. C. WARREN.

The following experiments were undertaken in connection with some other series, with a view to determining the manner in which conflicting data from different senses are harmonized. As they have, in addition to this, a special bearing on the sense of rotation, it seems best to give their results separately. The particular object of this investigation was to determine the relative influence of sight and the internal sense of rotation on the subjective estimate of movement. Messrs. W. J. Shaw and G. A. Tawney kindly acted as subjects during the entire series, which consisted of sittings about 20 minutes long, twice a week, extending over a period of four months in the early part of the year 1894.

The experiments were conducted in a dark room, 8x8 ft. The subject lay at full length on his back on a rotation-board, with the head somewhat raised and eyes so screened as to permit of his seeing only a small area of wall in front. At the foot of the board, and covering entirely his restricted field of vision, was a large screen with an aperture eight inches square, behind which a mirror could be hung. On two opposite walls of the room were hung strips of white paper, an inch wide, reaching nearly from ceiling to floor, and placed about a foot apart, which could be seen plainly in a very faint light, when no other outlines or shadows were

[1] Since revising the proofs of this article I have received a note from M. Flournoy in which he says: "Je suis, d'une façon générale, d'accord avec vous sur l'influence du type d'imagination" (making reference to my article in the *Medical Record*).

distinguishable on the screen or wall. When the subject closed his eyes, or the board was turned so that no strips were visible, the usual phenomena of rotary sensation were observed; the least discernible movement was 1° a second; a movement greater than this, if continued, gradually ceased to be noticeable, and any change in the rate was then interpreted as a new movement starting from a state of rest. But when the subject, believing himself to be at rest, was turned so that the strips came into view, a conflict arose between the internal sense and the visual sense, which had to be reconciled by some mode of interpreting the data. The experiments included trials both with and without the mirror, the subject never knowing whether the mirror was in or not.[1]

(1) With the minimum light by which the strips were discernible, the sense of sight played no rôle whatever in the judgment, the strips appearing sometimes to flit across the field of vision, and sometimes to move with the subject, according to the data furnished by the internal sense. (2) With an illumination ranging from one to six candles, visual impressions strengthened the internal sense of movement when they agreed, or tended to inhibit it when the conflict was not too great. Thus, with the mirror removed, the sight of the strips made even the slightest movement perceptible, and checked the sensation of reversed movement which occurs after a real movement ceases. With the mirror in, the least perceptible movement was between 1° and 2° per second. Movements greater than this were usually interpreted (when the strips were visible) as lateral movements of *progression in the direction in which the head was actually moving*. A sudden stop converted this into a judgment of *rotation in the opposite direction*. Sometimes, however, especially with very rapid movements, they were

[1] One of the subjects (T.) did not know of the mirror at all until the series was nearly completed. S. generally could not tell whether it was in or not; I questioned him at the end of each sitting, and found that, as a matter of fact, he never considered it in making his judgments, being too busy observing and reporting. T. was more inclined to dizziness than S., and his experiences were consequently less distinct and his answers less uniform. In general the two subjects agreed. I also confirmed a number of the results here given by experiments on myself.

interpreted as rotary, and the strips were declared to be moving also, but faster than the subject. (3) With a bright gas jet burning, the corners of the walls and many other outlines were plainly visible, and gave more general indications of a stable environment than the strips afforded. The judgment of *progressive movement* now occurred uniformly, except when the board was started with a sudden jar; the subject was unable to rid himself of the illusion; he would seem to be moving steadily sideways, even though he knew the impossibility of doing so in a small room. The writer confirmed these results personally. (4) In the final series the subject sat upright at the center of the board, with his head slightly in front of the center of rotation. In this position the interpretation of the movement as progressive is impossible, if the internal sensations in the head are regarded; and as a matter of fact no such judgment was given. Careful and repeated measurement by the metronome gave the following results, with the mirror in and a very strong light (two gas-jets): (*a*) Movements of less than 2° per second were judged to be objective merely. (*b*) Movements of 2°–5° per second were interpreted as subjective, but in the *reverse direction* from that in which they were actually occurring; *i. e.*, they were felt as movements, but their direction was determined not by the internal sense, but by sight, and they were thought to be in the opposite direction on account of the mirror. (*c*) Movements greater than 5° per second were interpreted correctly, with the remark that the wall was moving also in the same direction as the feet, and faster. The transition from (*b*) to (*c*) was very distinct; several times the speed was varied during the trial from greater to less than 5°, or vice versa, and each time there was an immediate change of judgment as to the direction of the movement. A single 'trial' in this case was always limited to 10 sec., in order to avoid the feeling of 'slowing up' which accompanies uniform movement.

Aside from the last-mentioned phenomenon, which belongs more properly to the general subject of conflict among sense impressions, the most noteworthy result was the transformation of rotary into progressive movement, by means of

visual data. Suppose the actual movement to be clock-wise. The head moves to the left, the feet to the right, and the strips and wall reflected beyond the feet are carried much faster to the right. But only the difference between the rate of the feet and the rate of the back-ground is taken into account; the back-ground is considered stationary, and the movement is interpreted as a movement of the subject bodily to the left. We may infer from this that the end-organ of the internal sense of rotation is in the head alone, since movements of the lower extremities are open to such absolute misjudgment. We are also led to the conclusion that the organ for the sense of rotation is the same as that for progressive movement. This is directly contrary to the conclusion reached by Delage, who denies that the sense-organ for progressive movement is in the head, while admitting it for rotary movement.[1] These results, moreover, seem to favor the view that the semi-circular canals constitute that organ, in spite of the objections recently raised by Ayers and others on morphological grounds.[2] A special sense-organ seems requisite for such a sense, and the above results tend to locate that organ in the head. In our experiments the sense of sight was made to furnish data of movement independent of the internal sense. In the *head* the latter sense was so strong that, within the given limits of visual field and luminosity, a conflict between the two senses invariably resulted in its favor, and any movement observed merely visually was attributed to the objects in the environment. Yet when the *lower extremities* were moving quite rapidly in one direction they were constantly declared to be moving in the other, on the testimony of visual data. The 'internal sense' of movement must therefore be due to something other than the general indication furnished by the vaso-motor system, which would affect all parts of the body alike.

[1] Physiologische Studien über die Orientirung, von Delage; deutsch von H. Aubert; pp. 94–95.

[2] Journal of Morphol. 1892, VI, pp. 237–256.

SHORTER CONTRIBUTIONS.

THE 'HAUNTED SWING' ILLUSION.

BY R. W. WOOD.

I was much interested this summer in the curious sensations produced by a purely optical illusion, known as the 'Haunted Swing,' at the Midwinter Fair in San Francisco. On entering the building we found ourselves in a spacious cubical room, furnished with a sofa, table, chairs, etc., a massive iron safe, and a piano, together with other minor articles. But the most conspicuous object was the huge swing, capable of holding forty or more persons, which hung in the centre, suspended from an iron cylinder which passed through the centre of the room. We took our seats and the swing was put in motion, the arc gradually increasing in amplitude until each oscillation carried us apparently into the upper corners of the room. Each vibration of the swing caused those peculiar 'empty' sensations within which one feels in an elevator; and as we rushed backwards toward the top of the room there was a distinct feeling of 'leaning forward,' if I can so describe it—such as one always experiences in a backward swing, and an involuntary clutching at the seats to keep from being pitched out. We were then told to hold on tightly as the swing was going clear over, and, sure enough, so it did, though the illusion was not so perfect as the high oscillations.

The device was worked in the following way: The swing proper was practically at rest, merely being joggled a trifle, while the room itself was put in motion, the furniture being fastened down to the floor, so that it could be turned completely over. The illusion was good, though the absence of centrifugal force, and the fact that the swing did not move with uniform acceleration as it descended, would indicate to a careful observer that he was not swinging freely. The curious and interesting feature however, was, that even though the action was fully understood, as it was in my case, it was impossible to quench the sensations of 'goneness within' with each apparent rush of the swing. The minute the eyes were shut the sensations vanished instantly. Many persons were actually made sick by the illusion. I have met

a number of gentlemen who said they could scarcely walk out of the building from dizziness and nausea. I myself experienced no sensations of dizziness, being accustomed to heights and to rapid motion; but the sensation before described was always present (and I visited the place several times), though I tried to suppress it and reason against it.

Heat Sensations in the Teeth.

BY HENRY RUTGERS MARSHALL.

In the course of a late operation upon one of my teeth I experienced a very powerful and distinct sensation of heat whenever the dental instrument touched a very thin layer of the tooth substance (dentine) which still remained protecting the 'pulp' from exposure.

The well-known Dr. Frank Abbott, who operated upon my tooth, and whose long and wide experience enables him to speak with authority, assures me that this sensation of heat is entirely independent of the temperature of the instrument employed: that in his experience he finds that any mechanical irritation of the dentinal fibers, when inflamed, will produce this sensation of burning, it being especially marked when the fibres are dragged asunder by the revolving instruments often used. The same heat sensation is produced, he tells me, by the rapid absorption of moisture produced by placing against this highly sensitive tissue a bit of 'spunk' or 'bulbulous paper,' or other rapid-absorbing substance. The substance called 'spunk,' which he uses for this purpose, is supposed to be nothing more than a tree fungus of especially fine fibrous nature.

It is apparent that we have here a production of heat sensations by stimulations which do not correspond in any evident way with the stimulations that produce heat in the 'heat spots' on the surface of the skin. I think it well to make note of these particular dental experiences in order that those who may be investigating the nature of the processes involved in the production of our sensations of heat and cold may upon occasion verify them, and may coördinate them with the more familiar means of heat production in the formulation of their theories.

DISCUSSION.

RECENT DEVELOPMENTS IN THEORY OF EMOTION.

Considerable interest attaches to the recent articles on Emotion by Professors Baldwin and Dewey. Both these writers are in favor of the James theory, but each has arrived at his results in his own way, and has his own view with regard to the weak points of the original statement of the theory and the value of the emendations that have recently been made. I do not propose, however, to deal with all the points of interest here involved. I shall merely discuss briefly the main arguments brought forward.

Professor Dewey contends that "all so-called expressions of emotion are in reality the reduction of movements and stimulations originally useful into attitudes" (PSY. REV., I, 6, 569). They are explicable by reference to useful acts, and therefore cannot be deduced from an antecedent emotion. Now, the term 'expression of emotion' is ambiguous, not only in denotation, but also in connotation. Few would assert that the bodily changes usually signified by this mode of speech are primarily intended to show that a certain individual has a particular emotion. The majority of psychologists would agree that the movements in question are in the main teleologically conditioned. The question is, whether you can conclude from this that they cannot be regarded as the outcome of some antecedent emotion. Professor Dewey's argument depends on the assumption that no useful action can be explained by reference to emotion. But surely the natural supposition is that so prominent a fact of consciousness has some function, and therefore directly or indirectly influences our actions. Professor Dewey himself says that "hope, fear, delight, sorrow, terror, love are too important and too relevant in our lives to be in the main the 'feel' of bodily attitudes which have themselves no meaning" (PSY. REV., I, 6, 563). It is hard to see how these can be important or relevant in our lives, if it is enough to dissociate an action from them to show that it has a purpose or end. I do not assert that all the bodily changes, internal and external, are the result of emotion. My contention is that it does not follow, merely because movements are

purposive, that they have no connection with emotion; and, further, that it would be a strange thing if this conclusion were correct.

We must now consider how far the detailed account of the bodily changes supports the 'effect' theory. A distinction is drawn, implicitly at all events, between the internal organic disturbance and those outward movements which are actions in the true sense, since they are directed towards some end. With regard to the latter, no explanation is given. All that is done is to show that they are directly teleological. It is first assumed that emotion can have no connection with useful actions, and then actions of this sort are simply taken for granted. If, however, they cannot be deduced from the emotion, it is legitimate to demand some account of their origin. It cannot be too strongly emphasized that the so-called 'cause' theory has an intelligible explanation to offer, while the 'effect' theory is silent on the point. Further, as Professor Baldwin brings out so clearly, it is not easy to see how any explanation can be given, on the principles of the latter view, which will fit into a theory of development.

A complication is introduced by the fact that some of these movements, such as the crouching of fear and the clenching of the fist in anger, are reflex. It is easy to understand, however, that once these actions have been voluntarily performed under the influence of a certain emotion, they arise reflexly in circumstances similar to those with which they were first connected. The identity of circumstance redintegrates the old movements, and so emotion and action appear simultaneously. In these cases, of course, the bodily changes are not caused by the particular emotion which they accompany. On the other hand, these reflexes are not the only 'expressions' of emotion. The original expression of anger, for instance, was doubtless a blow. The clenching of the fist points to restraining influences. But, though we do not now use crude physical measures exclusively, civilization does not leave us altogether helpless. The covert sneer, the insulting stare, the cutting remark are at our service. These and similar purposive actions require to be accounted for. The natural thing is to regard them as the effects of the emotion. There is an intelligible relation between them and the psychical state. An individual possessed with hate of some person will tell you that 'he feels as if he could'—do him all sort of injury. It is reasonable to suppose that this feeling conditions the action of retaliation. When it is asserted that this peculiar feeling towards the person only arrives after the action has actually taken place, we are at liberty to entertain a doubt in the

matter. When we find that no explanation whatever is given of the appearance of the action, our doubt will scarcely be diminished.

The organic changes, then, which are directly teleological, must be regarded as falling into two classes, namely, reflexes and voluntary movements. As Professor Baldwin shows, the effect-theory does not and cannot afford any explanation of the origin of the former in the first instance. The latter can scarcely be said to be taken into account at all, for the view is not worked out in detail. Cases like fear are used as instances where the action is practically the same under all circumstances, so that the reflex and the voluntary movements coincide.[1]

The internal organic changes remain, and Professor Dewey really faces the question here by attempting to show how they can be accounted for without reference to an antecedent emotion. They result from "the effort of the organism to adjust its formed habits or coördinations of the past to present necessities as made known in perception or idea. The emotion is psychologically the adjustment or tension of habit and ideal, and the organic changes are the literal working out in concrete terms of the struggle of adjustment" (PSY. REV., II, 1, p. 30). The habitual reaction does not harmonize with the mode of action which the present circumstances seem to demand. Hence there is a conflict, and, as a result of this, disturbances of visceral and associated organs, "which is just what we might expect when there is a great stirring up of energy preparatory to activity, but no defined channel of discharge" (PSY. REV., I, 6, 565). It is not easy to understand why the habitual reaction should assert itself so strongly in circumstances where it is obviously so much out of place, and insist on being 'coördinated' with the new mode of behavior. Nor is it obvious how emotion can be the 'tension of habit and ideal,' prior to action, when it is first constituted after the action has taken place (II. 1. pp. 18, 22). Leaving these points undiscussed, let us ascertain the results which follow from the explanation of the internal organic agitation here given. In the first place, the course taken by the deflected energy would seem to be mechanically determined as Professor James suggests (*Principles of Psychology*, II., p. 482). It will be dependent on the individual organism and its state at the moment. Professor Dewey is quick to see the effect of this. "If the bodily attitude is wholly accidental, then the emotion itself is brute and insignificant, upon a theory which

[1] In this connection one might safely venture the assertion that the theory under discussion would not seem so convincing if it were applied all round, instead of being stated generally, and merely illustrated by one or two favorable instances.

holds that emotion is the 'feel' of such an attitude" (I, 6, p. 563). He finds it 'more or less intolerable' that any idiopathic effects should be 'purely mechanical outpourings through the easiest available channel,' and maintains that the easiest path is determined by habits which, upon the whole, were evolved as useful (I, 6, p. 563). It would be very difficult, however, to prove that the bodily excitement could ever have been useful; and until such proof is offered, the 'intolerable' supposition of Professor James must be regarded as the more probable.

In the second place, under whatever circumstances the energy is aroused, the same amount spreading through the same organism at the same time will have the same effects. There is simply so much energy which is under a mechanical necessity to find an outlet. That the special occasion has no influence in determining the actual channels of discharge is rendered more obvious when we remember that the whole process is necessary just because action, appropriate to the particular case, has been inhibited. In every emotional state of the same intensity, therefore, the physical agitation will be practically the same; and this is a result more than serious to those who assert that the bodily changes cause the emotion. When the alleged causes are so much alike, why should the psychical effects be so widely different?

I do not criticise Professor Dewey for attempting to account for the internal organic disturbance without reference to emotion. It seems to me that he has shown that the phenomenon in question cannot be regarded as an effect of the emotion; and, further, that he has indicated the right principle[1] by which its origin is to be understood. My point is that the consequences have been shown to be disastrous to the theory he has adopted. For instance, one of the great difficulties which has to be met is that the bodily changes do not vary sufficiently in the case of different emotions, while they vary too much in different instances of the same emotion. The objection on this score was formerly made on the ground of empirical observation. Now, we can not only urge that the fact is so, but give a reason why, in the nature of things, it should be as it is. The escape of deflected energy is the cause of the internal effects, and these form the greater part of the whole mass of bodily change. Since the process is under mechanical law, the effects vary with the state of the organism; and this accounts for the fact that the same

[1] The principle must be worked out more fully, however, and freed from irrelevant additions. In all probability, too, it will be found necessary to supplement it to some extent by others.

emotion may at different times have different physical accompaniments. Further, under any outward circumstances a given amount of energy will always produce the same results if the inward physical conditions remain unaltered; and this explains why, in all states of equal intensity, the organic changes are substantially the same.

On the other hand, it seems possible for opponents of the effect-theory to state their views, so as to include and harmonize all the facts. You cannot proceed on the assumption that emotion must be either the cause or the effect of the physical changes. In the first place, the term 'physical change' is wide and vague. It covers movements, purposively reflex, voluntary, and mechanically determined. In the second place, the antithesis is false, for there is a third possibility, namely, that the psychical and physical aspects are independent of each other and yet concomitant. It is possible to hold, therefore, that some of the bodily changes are effects of the emotion, that others are independent of it, and that the emotion is in turn independent of the latter. When danger threatens, for example, it is possible to imagine that the perception is followed at once by fear and an arousal of energy, while certain actions or tendencies to action are called forth by association. All these arise simultaneously. We have at once the emotion of fear, the excitation of energy, and the tendency to crouch or run away. In the case of the first emotion of the kind, the third effect would not be present as a direct consequence of the cognition. The psychical state is a feeling with reference to the impending event prompting us to action of a certain kind with regard to it. The energy excited renders it possible for this action to be carried out with speed and vigor. If flight is out of the question, the energy diffuses itself through the organism and produces the violent organic paroxysm of intense terror. If the danger can be avoided and the means are suggested, the energy is used up for the most part in carrying out the necessary movements. Still a certain quantity always discharges itself through the body, for more energy is aroused than is absolutely necessary, and in most instances an interval must elapse before means can be found and the action put in train. Even though the purposive action is not inhibited, therefore, a certain degree of physical agitation is always present. I have purposely taken one of the cases which is most favorable to the effect-theory. The organic perturbation is not nearly so marked when, as in hate for instance, immediate and decisive action is not so vitally essential. The emotion may be as strong, however, for the physical changes vary in strength and prominence, not with the psychical concomitant, but

with the practical demands of the situation. Further, it is only in an instance, as simple as the one I have chosen, that it can be maintained with any show of plausibility that reflex action accounts for all the movements involved. In more complicated cases more complicated actions are necessary, and it is not so easy to exclude from consideration the influence of the purely psychical aspect of the concrete emotional state.

I can only refer to Professor Baldwin's interesting paper in so far as it bears on the special point now under discussion. This writer argues that "though habit means subsiding consciousness, it is just those 'expressive' reactions which are most instinctive, that carry with them most of the vivid and disturbed consciousness we call emotion" (Psv. Rɛv., I, 6, 612). Hence he concludes that the consciousness in question cannot arise until the instinctive reactions are reported back from the periphery. This view is open to criticism on many points, but I merely wish to point out that, thanks to Professor Dewey, it can be attacked on a question of fact. The argument depends on the assumption that all 'expressive reactions' are instinctive. Some are undoubtedly, but the greater number of them are caused by the deflection of energy or the spreading of excess energy through the body. They are not instinctive at all, but simply 'mechanical outpouring through the easiest drainage channels.'

We find, therefore, that nothing has been adduced on the basis of which it can be denied that emotion has a function of some kind, and causes some of the bodily movements which enter into the complete emotional state. The voluntary actions which seem to follow naturally from the peculiar psychical attitude are either left out of account altogether, or confounded with the instinctive reactions, which are themselves simply taken for granted. The explanation of the internal organic disturbance is in principle sound. It involves consequences, however, which are fatal to the effect-theory, and incidentally it destroys the special argument on the strength of which Professor Baldwin is induced to give a qualified assent to Professor James' main contention. On the other hand, opponents can accept it gratefully, and take account of it in framing their own theories.

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A REPLY.

If it is not contrary to editorial rules, I hope I may be allowed to protest against the unfair and dogmatic verdict which Professor G. S. Fullerton passes on the concluding section of my Aristotelian Address, 'The Conception of Infinity,' reviewed by him in the January number of this journal (Vol. II, Part I, p. 99), inasmuch as it is founded on a confusion of ideas which he attributes to me, though it is really due only to himself. The first part of his article is appreciative and generous, and for that I thank him. But when he comes to the sixth and last section of my address, which he calls my conclusion, he says that I reach it only by 'blowing both hot and cold in my premises,' which, supported as it is by a page of very plausible (though of course unintentional) misstatements, is an intolerable charge.

In three several cases on that page (99) he confuses distinctions which I clearly make and adhere to. He confuses:

1st—Between my 'material world' and my 'universe.'

2nd—Between my 'material element in consciousness' and my 'matter' or 'material world.'

3rd—Between my 'perceptually known' and my 'positively known.'

Now, in my argument, time and space are never perceived or represented in thought without some kind of 'material element in consciousness,' and in this sense the 'material element' shares their infinity; the 'formal' and 'material' elements together being indispensable constituents of anything thought of as existing (even when it is a void that is thought of), and therefore of the infinite universe.

An infinite void when thought of as existing, either in time or space, is a void only in the sense of being empty of physical matter, and I have given some reasons in my address for thinking that the world of physical matter is finite, being bounded by a void in this sense, both in time and space.

When we know any specific content (under which term physical matter is included) existing in time, or in time and space together, that is what I call having a 'positive knowledge' of it. But though we have no such positive knowledge of anything beyond the limits of the world of physical matter, we may yet have a 'perceptual knowledge' of time and space beyond them, namely, a representation of them as filled with some 'material element in consciousness;' this element being an indispensable condition of them as existing, although it is not specifically known to us.

The inseparability of 'formal' from the 'material' element in consciousness compels us to think of the universe as infinite, so soon as we recognize that one of these elements is infinite, and cannot be thought of as being otherwise; unless we assume dogmatically that only *that* really exists which man has specific sense-faculties to perceive. It is this dogmatic assumption which really 'shuts us up to the world of matter.'

This argument of mine may or may not commend itself as conclusive; but at least it cannot be charged with attaching varying and inconsistent meanings to the premises employed, which is the charge made in the words I have quoted from Professor Fullerton, and made in the most peremptory fashion.[1]

LONDON. SHADWORTH H. HODGSON.

A NOTICE.

Dr. Herbert Nichols, formerly assistant of the Harvard Psychological Laboratory, publishes in the last number of the *Philosophical Review* a most violent attack on that laboratory under the title 'The Motor Power of Ideas.' His scientific motives, to which I must confine myself here, are based on such an absolute misunderstanding of our paper and his discussion is so full of misstatements that I consider it useless and hopeless to attempt to correct his mistakes and discuss his arguments. But there is one point which troubles me. The paper which he refers to is published by Mr. Campbell and myself. Those who read the kind words with which Dr. Nichols praised my work a short time ago may not believe that he can change his mind so suddenly, and some may suppose perhaps that the attack is directed especially against Mr. Campbell. Under these circumstances I take it as my duty to free my young friend, Mr. Campbell, publicly from all responsibility. Mr. Campbell carried out the experiments most carefully, but the whole plan of work was laid out by me, and I myself wrote every line of the article. It is probably superfluous to add that I should have written every line just as it now stands even if I had known Dr. Nichols' so-called criticism beforehand. HUGO MÜNSTERBERG.

HARVARD UNIVERSITY.

[1] [A farther comment from Prof. Fullerton on Mr. Hodgson's positions will appear in the next number.—Eds.]

PSYCHOLOGICAL LITERATURE.

DEGENERATION AND GENIUS.

Dégénerés et Desequibrés. J. DALLEMAGNE. Brussels, Lamertin; Paris, Alcan; 1895. Large 8vo., pp. 658.

Dr. Dallemagne writes a very fluent colloquial style, well suited to the lecture-room, for which his book was originally written, and for this reason his pages, though immense, can be read quickly. The work is so full of details that our notice can hardly do more than recommend it to any reader who may be in search of a *vue d'ensemble* of the psychopathic constitution, and who at the same time has a good stomach for the great richness of the subject, and for the parenthetical sallys and sidepockets of reflection and description into which our author will drag him remorselessly. M. Dallemagne's reading has been enormous, and, so far as appears, exact; and it has in no way stifled his independence of judgment. His conception of the sphere of 'loss of balance' in the mental system is wider than that of Magnan, embracing Neurasthenia, Hysteria, and Epilepsy, of all which conditions the accounts he gives are both full of matter and up to date. Epilepsy, indeed, is the first subject treated after Imbecility, which latter follows six preliminary chapters on the psychological mechanism and on the notion and significance of the 'degenerative' type. The author allows that the conception of degeneration is still in process of evolution towards exactitude. It must indeed be admitted that causal and symptomatic notions mingle in it; heredity is an added element which is neither cause nor symptom; and a placid myxœdematous *crétin* has superficially nothing in common with a one-idea'd fanatic or other member of Magnan's class of *degenérés supérieurs*, except that both are 'queer.' M. Dallemagne has wrestled copiously with the problem, and perhaps better than anyone has emphasized the notion of *balance* as the test of mental health. Dissociation of the mental system, impulses so strong that they bear all inner opposition down, aversions and appetites at variance with the subject's beliefs, abrupt explosions interruptive of the consistency of his life, such are the traits of whole groups of psychopathic persons. M. Dallemagne even tentatively suggests a theory for such *disequilibration*. I find it very obscure,

but it suggests to my mind something like this: The thalami and corpora striata are 'subconscious' centres for habits, cravings and impulses that are not so 'saturated' with experience as to have become fatally automatic, like those in the cord. They normally act in coöperation with the fully conscious cortex and its associations of ideas. But they may become the seat of irritable weakness, and the associative cortical processes may be pathologically blocked or twisted from what is normal. In such cases the *poussée* from below is either excessive or ill-timed, and it may also fall on wrong ideas and the normally controlling ones be thrown out of gear. All sorts of obsessions, phobias, depravities of appetite, morbid impulses characteristic of the discordant self, which we find in the so-called degenerates, may be thus explained. In the 'inferior' class the cortex is most at fault; in the 'superiors' it is the basal ganglia.

Entartung und Genie, Neue Studien. CESARE LOMBROSO, gesammelt und unter Mitwirkung des Verfassers deutsch herausgegeben von Hans Kurella. Leipzig, Wigand, 1894. 12°, pp. 308.

A collection of essays and fragments, not published as a volume in Italian. The author first replies to some objections to his theory that genius is a degenerative neurosis allied to epilepsy and moral insanity. Dante, Michelangelo, and Guido had been thrown at him as examples of men of genius who were normal. He proves minutely their strongly eccentric and neurotic constitution. Dante in particular must have been frankly epileptic, for no less than eleven times in the divine comedy he speaks of himself as swooning or falling unconscious. That the weakness of genius cannot be due to secondary strains and fatigues incidental to the ardent sort of life which the possession of genius imposes, is proved by the fact that out of 313 symptoms of fatigue which Lombroso has counted, only six are commonly found among geniuses. Genius and sex is discussed in a chapter, full of anecdotes, on the conditions productive of genius. In Chapter III. is shown the frequency of degenerative anomalies in geniuses. For example, they vary from their national type, as is proved by portraits. Longfellow, Bellamy, Tennyson, Coleridge look like men of Latin race. Darwin and Bryant, Coleridge and Burns, George Eliot and Bulwer form mutually resembling pairs. "The cause of these resemblances is to be sought in the degeneration common to them all." Prof. Lombroso has compared the field of view of twelve geniuses of his acquaintance with that of eight unusually gifted young men who were not geniuses, and has found a shrinkage of the inner upper quadrant in nine of

the geniuses—in none of them was the field symmetrical. The non-geniuses were much more normal; so that an abnormal field of view seems to characterize genius. On the other hand, genius would seem to have, if anything, a slower reaction-time than usual. Amongst the *bizarreries* of genius, playing with orthography is mentioned, and a dog-latin letter of Swift to Stella is quoted. "One is tempted," Lombroso remarks, "to find in this tendency to fabricate a jargon, a trait connecting genius with criminality." The most valuable part of the book is constituted by biographical details concerning certain 'borderland' cases, calculating geniuses, thought readers, artists, and political and religious 'mattoids.' The author's curiosity and information, frankness, good-humor and vivacity are beyond praise, but his incapacity for accurate reasoning is apparently incurable; and this book, were it not for the biographic material which it contains, could only be regarded as one of the oddities of scientific literature.

Degeneration. MAX NORDAU. Translated from the second edition of the German work. New York, Appleton, 1895. 8vo., pp. 560.

A pathological book on a pathological subject. If one were to apply Herr Nordau's method to the description of his own person, one could hardly help writing him down as a degenerate of the worst sort. He is a 'graphomaniac'; a misanthrope and a 'misoneist'; a 'coprolalic' ('idiot,' 'imbecile' are his mildest terms of endearment); an 'erotomaniac' of the prudish sort, haunted by horror of other people's sexuality; an *obsédé*, pursued without respite by images of odious works of art; a 'megalomaniac' of the arrogant and insulting type; and, finally, a victim of insane delusions about a conspiracy of hysterics and degenerates menacing the moral world with destruction unless the sound-minded speedily arm and organize in its defence. Add to this equipment the earnestness of the gloomily insane, and their complete inability to see a joke (pages of heavy invective against Oscar Wilde's epigrams!) and one gets a not altogether consoling diagnosis of Herr Nordau's case. On the other side, it must be admitted that he is really learned, not only in contemporary German, French and English *belles lettres*, but in the literature of neurological medicine as well, and that many of the objects by whose odiousness his imagination is afflicted, Parisian 'pornographic' novels, for example, are loathsome indeed. When, however, hardly a contemporary name, however great, escapes his abuse, and the course over which he runs-a-muck lies through Wagner, Tolstoi, Ruskin, Burne-Jones, Rossetti, Zola, Ibsen, and Niet-

sche, as well as through Baudelaire and his descendants, it must be admitted that his volumes are little more than a pathological 'document' on an enormous scale, and an exhibition in minute detail of an individual's temperamental restrictions in the way of enjoying art.

The only chapters that concern this REVIEW are those entitled 'the psychology of mysticism' and 'the psychology of egotism' respectively. Mysticism and egotism are the great mental stigmata of hereditary degeneration. Mysticism is, in brief, the tendency to see in everything more than appears on the surface, and to suppose mysterious significance in the plainest facts. Its condition, our author says, is an inability of voluntary attention to confine the flow of association. In the exhausted and aimless brain of a degenerate, beyond the clear and immediate associates of an idea, there surges up circle beyond circle of remote associates, pale and vague reverberations of distant ideas, which make all perceptions spectral and all judgments uncertain. This is much like saying that in a healthy mind thoughts should have no atmosphere, no overtones, no fringes —an opinion to which few will subscribe.—Herr Nordau's explanation of the egotism of degenerates is based on the observations of Sollier and others upon imbeciles, and of Lombroso upon criminals, showing obtuse sensibility of the skin and other perceptive organs. Whilst the outer world thus comes to them and their congeners imperfectly, the inner world, on the contrary, fills them with its clamorous impulses and obsessions; their enfeebled will cannot hold the balance, the line (arbitrary at best) between the me and the not-me is shifted, and the me fills the field of attention. This theory, also, though it has its ingenuity, is one which psychologists will hardly find completely satisfactory.

The translation, so far as I have examined it, reads fairly well. But the publishers have made 560 very vast and ugly pages out of the 1000 odd convenient pages of the two-volume original.

Genie und Entartung, eine psychologische Studie. WILLIAM HIRSCH. Leipzig; Coblentz, 1894. 8°, pp. 340.

It really reanimates one, after so much farce-comedy writing on the subject of genius, to come upon a book based on psychological analysis, logic, and common-sense. It would be well if all the admirers of Lombroso, Nisbet, and Nordau could be compelled to read Dr. Hirsch's admirable study, of which every page is interesting and acute. I can only quote general principles from it, leaving out details. In the first place, the author remarks, 'genius' is a sociological, not a psychological concept. The class of persons popu-

larly recognized as geniuses—poets, musical composers, musical executants, actors, painters, men of science, statesmen, soldiers, and devotees—seem at first sight to have nothing in common except rarity and originality. But originality itself is not a psychological conception. An Englishman appears in France as 'an original'; and so does a Yankee in England. The oddity of lunatics, due to recklessness and non-inhibition, has nothing inwardly in common with the fertility in novel ideas that characterizes geniuses. "In different fields [as the author shows by detailed discussion] we see that the most diverse psychological elements constitute the man's genius, and that qualities which in one case make its essence, are in others actually incompatible with its activity. There are, therefore, no definite psychological qualities common to all geniuses. One would seek in vain identical features in Bismarck and Paganini, in Mozart and Napoleon." Even within one type there are tremendous differences, Goethe and Beethoven, for instance, having been men of mood and inspiration; Schiller and Mozart, men of continuous activity independent of mood, working with will and critical reflection. The mental *elements* are identical in the sane and the insane, the difference between these classes of men being one of proportion and mixture. With strong obsessions one needs a strong will to keep sound, just as with a large body one needs large legs to keep active. The excessive sensibility of a Goethe would have made a psychopath of him, but for his extraordinary intellect and self-control—with these additions it simply made him the mightier pattern of mankind. The logic which, because it finds hallucinations in crazy people, treats them thenceforward as an insane symptom, even where no other insane symptom is present, begs the question. Their existence in sane men should on the contrary be held to prove that they are not necessarily a morbid symptom. The 'simple enumeration' of geniuses with psychopathic traits, to prove the essential psychopathy of all genius [apart from the fact that by the same logic one could prove that being born on a Sunday, or having brown eyes, was genius's essential condition], is carried out with no pretence to exactitude. Rightly used, the statistical argument ought to ascertain, first, the percentage of the mentally unsound in a given population at a given moment, then the total number of geniuses, and finally the number of mentally diseased geniuses, in the same population at the same moment. If the percentage of disease were higher in the geniuses than in the population, the neurosis theory of genius would receive presumptive support. It is needless to say that such statistics are unattainable, nor are they attempted

by any of the advocates of the neurosis theory.[1] The close of Dr. Hirsch's book has much to say *in ré* Nordau *versus* Wagner and Ibsen. This critical matter calls for no reproduction here.

If the reviewer might now say a word of the result left on his own mind by reading the genius-controversy, it would run something like this: Moreau, Lombroso & Co. have done excellent service in destroying the classic view of genius as something superhuman and flawless. By their ferreting and prying and general devil's advocacy, they have helped us to an acquaintance with human nature *in concreto*, which from every point of view is superior to our old-fashioned academic notions. Lombroso in particular has put us in his debt by his studies of individual fanatics and 'mattoids.' But there the service stops, for (except in Nordau's case) these authors are incapable of logical or psychological analysis; and the only conclusion that their facts make more clear than ever—the conclusion, namely, that there are no incompatibles in human nature, and that any random combination of mental elements that can be conceived may also be realized in some individual—is one that they do not draw. If we are to make of genius a psychological conception at all, it must be a property of *intellect* rather than of will or feeling. Narrowed in this way, Prof. Bain's description of it, as an unusual tendency to associate by similarity (a description with which none of our authors seem acquainted), will stand firm. But it is one thing to have this intellectual condition of genius and another to become effective in

[1] The only attempt to use statistics methodically by advocates of the theory is in Nisbet's comparison of the causes of death in genius and in the population at large (see "The Insanity of Genius," 1893, p. 315). This author says: "I have dealt specifically with some 250 men of genius. Selected upon no other ground than their eminence, in the first instance, the *total number* of these are found to be neuropathic, suffering from or dying of some description of nerve disorder." Mr. Nisbet then enumerates amongst the "chief constitutional or nerve diseases": Phthisis, pneumonia, apoplexy, heart disease, scrofula, rheumatism, syncope, diabetes, gout and stone, and then tells us that the death-rate of the 'nerve diseases,' thus Pickwickianly understood, to the entire death-rate of Great Britain in 1888, was as 1 to $3\frac{1}{2}$, whilst "among the men of genius enumerated it is at least three times greater." Ignoring the ridiculousness of Mr. N.'s classification of nerve diseases, and assuming that all the 250 geniuses really did die of them, not the total population, but some part of the population whose pursuits are intellectual, was the proper term of comparison. If Mr. Nisbet had looked up the personal and family history of 250 occupants of offices in the city, or of 250 active business men, and found less gout and apoplexy than in his geniuses, it would have been a really interesting fact, for these are diseases of sedentary life as such, and geniuses are on the whole of the sedentary class. Similarly before the 'family history' of geniuses can be held to prove the neurosis theory, a comparison must be made with the families of an equal number of business men, 'selected upon no other ground' say than their wealth. No such comparison has ever been attempted.

history *as* a genius, and to figure in biographical dictionaries. We all know intellects of first-rate original quality whose names are written in water because of the inferiority of the other elements of their nature, their lack of remote ideals and unifying aims, of passion and of staying power. On the other hand we know moderate intellects who become effective and even famous in the world's work because of their force of character, their passionate interests and doggedness of will. To *do* anything with one's genius requires passion; to do much requires doggedness. Hence it comes that the intense sensibility of the psychopathic temperament, when it *adds* itself to a first-rate intellect, greatly increases the chances that the latter will bear effective fruits. To be liable to obsession by ideas, not to be able to rest till they are 'worked off,' ought then to be, as they indeed are, traits of character often found amongst the men whose names figure as those of geniuses in the cyclopedias. But these traits have no essential connection with the sort of intellect that makes the men geniuses. We may find them combined with any sort of intellect, as we find first-rate intellect combined with any sort of character. The names of Emerson, Longfellow, Lowell, Whittier, and Holmes would probably be those first written by any one who should be asked for a list of the geniuses of the community in which I write. Although belonging to the class of poets (the species of genius most akin to psychopathy by the sensibility it demands), these men were all distinguished for balance of character and common sense. So Schiller, so Browning, so George Sand. In poets like Shelley, Poe, de Musset, on the other hand, we have the intellectual and passionate gifts without the powers of inhibition. In the sphere of action we have a similar diversity of mixture: we find the all-round men like Washington, Cavour, and Gladstone; the great intellects and wills with no hearts, like Frederic the Great; the intense hearts and wills with narrow intellects, like Garibaldi and John Brown; the stubborn wills with mediocre hearts and intellects, like George III. or Philip II.; and, finally, the real cranks and half-insane fanatics, often with much of the equipment of effective genius except a normal set of 'ideas.' It all depends on the mixture; only as the elements vary independently, the chances that a freak of nature in the line of human greatness will be as exceptionally strong in all three elements of character as he is in any one of them, is small. Hence some lop-sidedness in almost all distinguished personages, hence the rarity of the Dantes, St. Bernards, and Goethes among the children of men.

One more word: there is a strong tendency among these pathological writers to represent the line of mental health as a very narrow crack, which one must tread with bated breath, between foul fiends on the one side and gulfs of despond on the other. Now, health is a term of subjective appreciation, not of objective description, to borrow a nomenclature from Prof. Royce: it is a teleological term. There is no purely objective standard of sound health. Any peculiarity that is of use to a man is a point of soundness in him, and what makes a man sound for one function may make him unsound for another. Moreover, we are all instruments for social use; and if sensibilities, obsessions and other psychopathic peculiarities can so combine with the rest of our constitution as to make us the more useful to our kind, why then we should not call them in that context points of unhealthiness, but rather the reverse.

The trouble is that such writers as Nordau use the descriptive names of symptoms merely as an artifice for giving objective authority to their personal dislikes. The medical terms become mere 'appreciative' clubs to knock men down with. Call a man a 'cad' and you've settled his social status. Call him a 'degenerate,' and you've grouped him with the most loathsome specimens of the race, in spite of the fact that he may be one of its most precious members. The only sort of being, in fact, who can remain as the typical normal man, after all the individuals with degenerative symptoms have been rejected, must be a perfect nullity. He must, it is true, be able to perform the necessities of nature and adapt himself to his environment so as to come in when it rains; but being free from all the excesses and superfluities that make Man's life interesting, without love, poetry, art, religion, or any other ideal than pride in his non-neurotic constitution, he is the human counterpart of that 'temperance' hotel of which the traveler's handbook said: "It possesses no other quality to recommend it." We all remember the sort of school-boy who used to ask us six times a day to feel of his biceps. The sort of man who pounds his mental chest and says to us: "See, there isn't a morbid fibre in my composition!" is like unto him. Few more profitless members of the race can be found. The real lesson of the genius-books is that we should welcome sensibilities, impulses and obsessions if we have them, so long as by their means the field of our experience grows deeper and we contribute the better to the race's stores; that we should broaden our notion of health instead of narrowing it; that we should regard no single element of weakness as fatal—in short that we should *not be afraid of life.*

W. J.

GENERAL.

Thèorie de l'ondulation universelle; essais sur l'évolution. B. CONTA. Paris, Alcan, 1895. Pp. 216.

Perhaps the thing of greatest interest in this volume of metaphysics is the biography of the author. Conta, born in Maldavin, of illiterate parents, by turns conjuror and student, constantly changing his profession to make his living, but always pursued by the same sensations of hunger and phthisis, ended by becoming professor of law, and died Minister of Education of his country. Such a life proves that he was a man of unusual energy. As for his theories of universal undulation—which are not on the whole very different from the theories of evolution—they have very little interest for us, so schematic are they and distant from facts! Toward the end of his book there are some curious details on what Conta calls laws of universal assimilation. I say curious because the author, hastened and hindered by the malady which finally carried him off, had not time to develop his thought, and he has thrown together simple notes, and these notes seem to me to explain his method very clearly. It follows from the first idea that all bodies tend to assimilate others by each communicating its own peculiar external and internal movement. This applies in the first place to purely physical phenomena: so a body that presses (this is his own expression, p. 208) communicates the direction of its own displacement to that of the body that is pressed, etc. Then come the physiological phenomena: the particles which go to make bone are transformed into bony matter. Then he passes to contagion of diseases, without being aware of the abruptness of the transition: the different degrees of unhealthiness are communicated from individual to individual. Finally, the author cites, as being part of the same series, psychical influences. We experience the emotions of those with whom we happen to be; we laugh with those that laugh, and weep with those who weep, etc. All phenomena attributed to imitation belong to assimilation: "all phenomena produced in the nervous system of the person who influences is communicated with more or less force to the nervous system of the person influenced" (p. 211). I think that this series of arguments gives us enough light on the value of this work. A. B.

SORBONNE, PARIS.

La Vie et la Pensée: essai de concéption experimentale. G. PIOGER. Paris, Alcan, 1894. Pp. 260.

In this book M. Pioger aims to trace the development of organic life into conscious and mental life, and to this end he gathers and

coördinates facts from the experimental sciences. Hence he adds to his title the phrase 'experimental conception' or the systematization of our *real* knowledge. Such knowledge is confined to our thought of the relations which we find among things, for we cannot penetrate into the *substance* of things. Yet it is *true and objective* knowledge in spite of Berkeley (?); and this knowledge enables us to conceive the world experimentally—that is, to systematize the relations which we perceive and to embrace them as a whole by thought.

Our thoughts are produced by the special orientation of our psychic sensibility, which suffers the influences of the environment of which it is a part, and of which the most intimate parts also go to compose it. So thought is prepared for specific organic and vital functions. It appears at the moment that what M. Pioger calls *mentality* arises from the relation of certain vital elements. Mentality is that which *personifies* our intellectual aptitudes and gives us our mental constitution. This constitution, therefore, takes root in our organic constitution, and that in turn in our inorganic constitution—a result from the solidarity of inorganic elements. From mentality and its phenomena we reach the concept of consciousness, which is a mid-term between life and thought.

Consciousness, therefore, plunges its roots into the depths of our lower life and pushes its branches up into the intellectual and social. M. Pioger gives a table showing the various ramifications.

The individual consciousness is, then, only a preparation for social life and consciousness, whose phenomena arise from the reciprocal action and articulation of social elements, just as in turn the phenomena of mentality and intelligence result from the articulation in the sphere of the individual's psychic sensibility. To the first part, which gives the analysis of the elements constitutive of thought and life, and shows the lower regions in which they lie hidden, M. Pioger adds a second part devoted to the synthesis of elements. He shows the solidarity which they come to present in the individual (in thought), in the race (by heredity), and in society.

To sum up, the book is an attempt to throw together some of the data of experimental science from a point of view similar to Spencer's, but narrower. But the data are arbitrarily chosen, and the results are in many cases open to dispute. It is not based on original or new research. J. PHILIPPE.

SORBONNE, PARIS.

Peregrinazioni Psicologiche. T. VIGNOLI. Milan, Hoepli, 1895. Pp. 404.

This is a collection of notes and essays published on various occasions between 1882 and 1894. The title of the book is justified

not only by the variety of subjects it handles, but also by the lack of any studied arrangement, neither the order of time nor that of topics being strictly observed. Each article, however, is marked by lucidity of exposition and by a wealth of details which, though familiar for the most part to students of psychology, are made to do good service for the critical and constructive purposes of the author.

The volume would not have suffered seriously by the omission of the discourse on 'The Paleontology of the Spirit,' as this is merely a bit of sarcasm expended on unscientific notions—'fossils of the mind.' Of the remaining articles, that which possesses the most actual interest is on 'colored audition.' To understand this phenomenon we must recall the facts of brain-growth. The protoplasm was the seat of general sensation. As the various tissues, organs and centres were differentiated, the original aptitude to receive all sorts of impressions remained in a latent form. Its revival explains those 'organic metaphors' whereby a single impression gives rise to different sensations. This explanation, however, is put forward simply as an hypothesis, with a prudent 'perhaps' here and there.

'Paramnesia' the author handles with more assurance. Such peculiarities of memory, far from being abnormal, are accounted for by three causes: the reproduction, by association, of ideas, images and feelings; the rapidity of thought; and the automatic constructive power of mind and imagination. Because the mind, when it perceives an analogy between a present object and one that was previously perceived, fails to distinguish one from the other, it transfers the actual image to an indefinite past. The comparative judgment is inhibited partly by the rapidity of thought and partly by the unconscious character of one of the factors.

To 'certain unconscious intervals in a coördinated series of psychic acts,' a lengthy study is devoted. That such intervals are possible is shown from numerous facts of memory, dream-picturing and the ordinary activity of the waking state. They are filled in by cerebral functions, which, though they do not rise into consciousness, are capable, because of the laws of heredity, of linking one conscious state with another.

An inquiry into the 'genesis of our sense-perceptions' leads, by delicate analysis of the physical and physiological processes, to the vexed question—How does the brain-motion become sensation? The answer is facilitated by a comparison. Between the qualities which an element acquires in passing from one allotropic state to another, and its molecular structure, there is no relation that we can discern. Nor is there, so far as we can perceive, any relation be-

tween the physiological process and the sensation. They are two states. The physical state and the psychical state are the eternal and fundamental forms of the universe.

Five of the articles contained in the volume, though treating of different subjects, present a certain similarity. Thus the growth of 'the moral sense' is explained according to the laws of evolution, and especially of heredity. Man's vicious proclivities are the effects of atavism, of reversion to a pre-human condition from which man emerged by an act of reflection. To make the results of this act prevail over the atavistic tendency, is the secret of social progress. Again, 'attention' being widened out till it means 'response to a stimulus,' it is found to be essentially the same throughout the animal series. Only in man there is a power of introspection whereby he can attend to the very act of attention; and this it is that distinguishes him from the lower animals. The same line of demarcation is drawn in the article on the 'origin of articulate speech.' Man has, in common with other animals, a 'physiological language'; but this is fundamentally different from speech. The latter is not a copy but a symbol of the internal process. In man thought precedes speech; so that from the articulation of the one by reflection there results the articulation of the other—its outward expression.

The importance of the 'sensory image for the development and exercise of intelligence,' arises from the very vagueness of the image that would seem to be an imperfection. Our perceptions are generic, *i. e.*, they give us but a small part of the details which the object really contains. This is required in all animals by the necessities of existence; since life would be impossible if a minute examination of each object had to be made. In man, moreover, the generic character of perception aids intelligence and gives rise to science, by serving as a means of classification and ulterior abstraction. The act of reflection being proper to man, extreme caution must be used in interpreting those actions of animals which seem to be on a par with those of human intelligence. On this principle, and on his personal observations, Vignoli criticizes with considerable keenness the accounts given by Lubbock and others of the dog's reading and counting abilities, and shows, how in these respects, the animal is inferior to the child and the savage.

His 'notes on a psychology of sex' are divided into two parts. In the first he outlines the intellectual, moral and industrial traits by which the sexes differ, and which depend upon organic and functional differences. In the second he insists that man, from the

beginning, must have been a social being; otherwise, articulate speech would never have been formed.

WASHINGTON. E. A. PACE.

Philosophy of Mind: An Essay in the Metaphysics of Psychology. G. T. LADD. New York, Charles Scribner's Sons, 1895. Pp. XIV + 414.

It is a little more than a year since Professor Ladd ended the preface to his *Psychology, Descriptive and Explanatory*, with a promise to deal in a later work with the philosophical problems which empirical psychology suggests. This promise is fulfilled in the volume before us. The field covered does not include all the questions to which psychology gives rise—it would need a system rather than an 'essay' for that—but the author selects for treatment a number of topics which are intimately connected with current discussions, and which possess an abiding interest. The standpoint from which these are considered is that of the 'empirical science of mental phenomena.' As Professor Ladd says (p. 82):

"Indeed, this essay in the philosophy of mind is deliberately based upon previous long-continued researches into the facts and laws of a scientific psychology. * * * And it is the author's controlling wish that the validity of the following speculative conclusions should constantly be brought face to face with the conclusions of the empirical science of mind."

Here, whether it agrees with Professor Ladd's views or not, the psychological world will be a unit in according him the praise which is due the patient endeavor to base metaphysics on the only secure foundation. No one among us has more earnestly studied or more carefully sifted the data and the outcome of scientific psychology than Professor Ladd; no one, therefore, is better entitled to claim for his results the consideration which of right belongs to thinking of such a character.

Further, as psychologists are already acquainted with the author's empirical conclusions, so they will find his metaphysics familiar almost at a glance. In general it is distinctly Lotzean in tone; while Professor Ladd's special opinions have been foreshadowed in his earlier works, including his *Introduction to Philosophy*. Both these points are evident from the metaphysical preludes with which many of the chapters of the present work begin, as well as in the conclusions reached. For instance, reality is thus defined (p. 120):

"Every real being is known as a self-active subject of states, standing in manifold relations to other beings, and maintaining its right

to be called real by acting and being acted upon,—only, however, in obedience to certain laws (or uniform modes of its behavior as such a being and no other)."

Again, concerning the consciousness of identity, it is argued (p. 151):

"Every x (every 'Thing' whatsoever), in order to be entitled still to be called x (or the 'self-same' thing) must in its changes run only through series such as can be indicated by $x, x^1, x^2, x^3, x^4 \ldots x^n$; or on occasion of its coming into other relations with different beings, the series may be that indicated by $x, x^\alpha, x^\beta, x^\gamma, x^\delta \ldots x^\omega$."

And it is concluded:

"The real identity of any thing consists in this, that its self-activity manifests itself, in all its different relations to other things, as conforming to an immanent idea." Similarly unity in anything whatever is held to imply 'the presence of some ideal in the very being of the thing' (p. 191), and self-consciousness, in its unitary development, to yield the best, if not the only conception of what a unit-being is; permanency in things and minds alike is deemed a matter of inferred rather than of direct knowledge, and the permanent being of mind is believed interrupted when consciousness lapses, except for the modicum of existence which consists in 'a certain abiding relation to all reality' or 'the world-ground' (p. 392).

The interest of psychologists, therefore, will centre about the way in which these two familiar elements of Professor Ladd's thinking are combined and the results to which his inquiry leads him. The book opens with two chapters on 'Psychology and the Philosophy of Mind.' The chief thesis here is the impossibility of divorcing psychology and philosophy altogether. This will be admitted by all—as to the latter end; for that psychology issues in the problems which philosophy discusses, is not susceptible of doubt. That psychology as a science actually does, and of necessity must, include metaphysical assumptions in its course, should be equally clear; although, no doubt, many will question the truth of the proposition. It is an easy task for Professor Ladd to show that the natural science, on the level of which our 'new psychology' delights to stand, is itself 'shot through' with metaphysical elements; and just as easy, though the work is rather more novel, to prove by examples—Höffding, James, and Flournoy are cited—that the professed rejectors of metaphysics are among the chief offenders against their own first principle. The nerve of the argument appears, however, in the conclusion that the only legitimate choice left for the

psychologist is between the uncritical dualism of common life and the adoption of 'some definitive metaphysical point of view' (p. 42) of his own selection, as has been done by Volkmann and Wundt. This may be the alternative in the present transitional condition of psychology. But surely history points toward a better ideal for the future, namely, the critical—though not always reflectively critical—determination and adoption by all of such principles as will best subserve the progress and the exactness of the science. It was in this way that the rising sciences of the modern era threw off the trammels which formed their heritage from Aristotle and the mediævalists; thus physics, to take a more special example, has in our own time been criticising some of its fundamental concepts, although to students of philosophy its advance may seem painfully hesitating and slow. So also psychology is still in the throes of its new birth. And when the happy time shall come for us to be fitted out with even as good a set of *working principles* as that which the physical sciences of the day enjoy, we shall be secure from the vagaries of the 'psychologies without a soul' on the one hand, and the necessity of constant re-discussion of our primary assumptions on the other. But on any view of the case, it must be regretted that Professor Ladd's polemic manner lags behind the material force of his arguments. The use of horrible examples is always a dangerous expedient in a technical treatise; and the psychological world will unite in deploring the characterization of Höffding's introduction of metaphysics into his psychology as a 'covert effort' (p. 22) and James's positivistic attitude as the 'position of materialism' (pp. 28, 39).

Chapters III–VI constitute the kernel of the volume. The chief positive outcome of the first of them, on 'The Concept of Mind,' is the emphasis laid on the element of self-activity in all self-consciousness. Chapter V, on the 'Consciousness of Identity and So-called Double Consciousness,' is the paper presented by the author at the last meeting of the Psychological Association; together with Chapter VI, on 'The Unity of Mind,' it advocates identity and unity as real predicates of the self, on the basis of the unity of the life of consciousness and in the sense of the definitions above cited. Chapter IV deals with a question central to the whole discussion, 'The Reality of Mind.' Noëtically, it is argued here, 'knowledge implicates reality,' and, metaphysically, all the marks of reality belong to the mind, known as a 'here-and-now-being' in self-consciousness and as a 'then-and-there-being' in memory, and inferred to be a permanent existence by reflective thought working on the data of

experience. Hence, also, it is concluded as a corollary, "The peculiar, the only intelligible and indubitable reality which belongs to Mind is its being for itself, by actual functioning of self-consciousness, of recognitive memory, and of thought" (p. 147). Yet it is with a sense of disappointment that the reader ends the chapter. The difficulty is partly one of method. In putting his most important thesis so early in his work the author has lost the advantage of the several arguments, positive and negative, which later on might have been combined into a proof of cumulative force. It is partly a difficulty with Professor Ladd's theory of knowledge, at least in so far as he has yet announced it. After diligent study of his various works, the present writer inclines to the belief that his first principle, 'knowledge implicates reality,' knowledge and reality are correlates, etc., might '*in some sort*' be acceptable to many of those not agnostics or phenomenists. But when this is used as a kind of universal major, with little or no systematic determination of subordinate criteria, especially when the psychology and the noëtics of self-consciousness are so intermingled that it is often impossible to decide on the basis of which of the two the argument is proceeding; the effect is not only confusion concerning the meaning of Professor Ladd's reasonings, but doubt in regard to their validity. But the difficulty arises partly, also, from an underestimation of the strength of opposing theories. The same failure to realize the importance of the reinforcements which have come to the cerebralists and materialists from the newer researches that marked Part III of the *Physiological Psychology*, reappears in the present treatise. And this, though the psychological world owes a debt of gratitude to Professor Ladd for the earnest defence of the reality and spirituality of mind which he has given alike in the earlier and in the later work.

Chapters VII–VIII, on 'Mind and Body,' are for the most part an elaboration of chapters XI, XXI and XXII of *Psychology, Descriptive and Explanatory*. But chapters IX, 'Materialism and Spiritualism,' and X, 'Monism and Dualism,' are among the most successful in the book. In the former vigorous blows are dealt the materialistic theory, without yielding to the claims of spiritualism in the monistic sense of the term. In the latter a still more forcible assault is made on both the scientific basis and the metaphysical deductions of the current psychological Spinozism. In Professor Ladd's own words (p. 344):

"In brief, then, the alleged scientific principle of psycho-physical parallelism is far from being the self-evident conclusion of modern psycho-physical research which it is so often and so rashly assumed

to be. Even the simplest relations between the phenomena of the lowest order of consciousness and the concomitant cerebral activities are far too fluctuating, complicated and changeable to be subsumed under this principle. Of parallelism in space we cannot speak appropriately in this connection. Of parallelism in time there is only an incomplete and broken analogy. And when one tries to think out clearly the conception of a complete qualitative parallelism, one finds the principle soon ending in inadequacy, and finally becoming unintelligible or absurd * * *."

Nor if the doctrine of psycho-physical parallelism, in the fullest meaning of the phrase, were proven, would monism necessarily follow. Rather the clearest inference would be to a moderate dualism, even though it is difficult to share the author's confidence that the 'double-aspect' theory is utterly meaningless.

The remaining discussions of the 'Origin and Permanence of Mind' and the 'Place of Man's Mind in Nature' point forward to the future development of Professor Ladd's views on ethics and the philosophy of religion. These will be the more eagerly awaited in view of the value of the present volume, which, in spite of defects, is one of the most notable contributions of recent years to the literature of the subject.

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Elements of Psychology (*Syllabus of Philosophy I*). J. H. HYSLOP, Columbia College, New York, 1895. Pp. 130.

This syllabus is best described in Dr. Hyslop's own words: "As a time-saving instrument in my lectures on the subject, and as a guide to my students in their reading and study." It has also a personal interest in showing those who have been instructed by the author's work in other departments—logic and ethics—his general conception of the psychological area and its problems. Aside from these two uses it is hard to see what purpose it can serve. The analysis, while systematic and thorough as analysis, (excepting the chapter on emotion) does not allow one to see far enough into Dr. Hyslop's mind to warrant confidence as to one's insight into what his psychological position really is. This, of course, is a defect, if defect it be, not in execution but in original design ; for Professor Dewey has recently shown in his 'Study of Ethics' the possibility of a syllabus which shall contain both outline and suggestive doctrine. The analysis is indeed so thorough and comprehensive that here at least we believe that 'the part is not worth more than the whole,' and we hope that Dr. Hyslop may see fit some time to give his lec-

tures text-book form. The balance between the introspective and the 'extrospective' methods is, so far as can be judged, well preserved, and the author's breadth of view and psychological tolerance are well shown in his use of the observations of the 'psychic researchers.' How justifiable his particular use of these observations is, is another question (witness, for example, the remarks on the phenomena of retention), for the condensed analytic outline, because of its necessary meagerness, warns one off the field of interpretation and criticism. This is shown again in the remarks made on subliminal consciousness.

Particularly striking is Chapter X, on the 'Will or Conation.' Psychological students owe the author a debt of gratitude for this piece of analysis. It is a valuable supplement to the chapter on the 'Freedom of the Will' in the Ethics. These two chapters throw light on each other.

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Popular Scientific Lectures. E. MACH. Translated by Thomas J. McCormack. Chicago, The Open Court Publishing Co., 1895. Pp. 313. Price, $1.00.

Nearly all the lectures constituting this volume deal with the physiological or psychological side of physical questions; four treat of the methods and development of science, briefly outlining a theory of cognition.

Science, according to Prof. Mach, is essentially an *economy of thought.* Rooted in the most primitive psychical functions of life, this economy reaches its highest perfection in language and mathematics. Here, in the psychological origin and nature of our ideas, the elucidative power of the principle is obvious.

A natural law is a rule or body of directions for the mental reconstruction of facts; enabling us to anticipate and retrace in thought the steps of nature. To this end it embraces *only certain aspects* of the facts, such as are determinative. By means of these determinative elements and their formal constituents, we complete in thought *facts that are only partly given;* derive complete results from incomplete data. To reach the ungiven elements we should, on the primitive plan, have to resort to slow and tedious experience; that infinite pains we *save* ourselves by economical natural laws. This is all that science accomplishes. Its individual results we could reach in a sufficiently long time directly and without methods.

The mental reconstruction of facts we accomplish by *description,*[1]

[1] The view that 'explanation' is merely the description of unknown phenomena in terms of known phenomena was stated by Mach before either Clifford or Kirchhoff.

rendered necessary by communication, effected by comparison. Of description there are two kinds: direct and indirect. We describe a fact directly when we employ terms having an *abstract* import only, where our comparisons suggest only conceptual relations. For example, the definition of quantity of heat, being a definite numerical statement of a certain determinate relation between temperatures and masses, is a direct description, involving no adscititious notion, and having validity whether heat is a substance or a motion; and the same is true of energy. Now Black, in formulating the definition of quantity of heat, and Mayer and Joule, in stating the law of energy, viewed the facts under the notion of a *substance*; in so doing they resorted to *indirect description*—they employed a simile in which unessential and superfluous features were involved. Of such a character are *theories* in science, the wave-theory of light, heat as a motion, etc., useful in the preliminary steps of research, of unmistakable power as heuristic agents, but destined to be discarded when that final consummation of knowledge is reached—the *simplest and completest possible abstract expression of the facts.* Direct description is the goal of all research. Moreover, the method of physiological psychology is the same as that of physics.

Finally, is science, description, the ultimate unanalysable term in knowledge? Viewed in its higher collective relations, and qualified by the restrictions incident upon such a view, it is. We seek in philosophy an *integral* aspect of the universe; but our method is the *differential* method. It is in the latter that we must seek the foundations and conditions of our knowledge, not in the former. That economy which is embodied in our thoughts is conditioned upon the formal needs of the mind, but it is not necessarily to be found in nature.

T. J. McCormack.

SOCIAL PSYCHOLOGY.

(1.) *An Introduction to the Study of Society.* A. W. Small and G. E. Vincent. Am. Book Co., 1894.

(2.) *Les Transformations du droit.* G. Tarde. 2 ème ed. Alcan, 1894.

(3.) *L'Imitation et la logique sociale.* R. Berthelot, Revue de Metaphysique et de Morale. 1894. Pp. 93–97.

with whose names it is usually associated. The theory, natural enough in its origin, is not new in philosophy, although propounded independently by all the above-mentioned inquirers and rendered exact only by their definitions.

(4.) *Le Problème de la sociologie.* G. SIMMEL, idem. Pp. 497–504.
(5.) *Les Règles de la methode sociologique.* E. DURKHEIM, Revue Philosophique, May–August, 1894.

The province and method of sociology are at present a centre of discussion. As sociology in one of its aspects is social psychology, this involves discussion as to the possibility of a social as *vs.* an individual psychology, and, if this be admitted, as to the natural relations of the two.

1. The *province of sociology* is, according to Professor Small, threefold. Descriptive sociology is the science of the coördinated facts of society as it is; statical sociology is the science of social ideals, an approximate account of the society which ought to be; dynamical sociology studies the available resources for changing the actual into the ideal. Passing over the last two, descriptive sociology is more particularly "the organization of all the positive knowledge of man and of society furnished by biology, anthropology, psychology," etc., etc., and "attempts to combine the testimony of these special sciences into a revelation of the accidental and permanent factors in social combinations." The other writers are all inclined to seek a narrower, more specific field. M. Tarde (whose numerous works demand a special treatment) finds the essential characteristic of social phenomena to be 'imitation,' understood in the sense of "every reflection of one mind in others, of one will in others." Similar social phenomena (those of law are studied in particular) may be due either to 'invention,' in response to the demands of the environment, which refers us to a biological cause, or to imitation, a social cause. It is then this latter class of relations which forms the subject of a 'pure' sociology, as distinguished from biology and history. M. Berthelot, in an acute review of Tarde (Revue de Met., 1893, 507–518), of which (3) is a restatement, objects that to make imitation the sole social category is a mistake like that of the Ionic school of philosophers with their water, air, etc. "The object of a 'pure sociology' is to determine the conditions apart from which no stable social group is possible." Sociology is social logic, a theory of the inventions necessary for society; and imitation, like language or law, is merely *one* of these necessities. In fact, it falls under the consideration of pure sociology only in proportion as it is shown to be necessary to this end. Professor Simmel attempts a still more definite delimitation. If sociology embraces all that occurs in society, and is understood as an explanation of all events by social forces and considerations, it is only a method (as, *e. g.*, induction), not a special science. But as psychology separates

out the content of mental states and considers simply the form, so sociology must isolate the distinctively social and study the *form* of association as such. The contents, *i. e.*, the interests and objects realized by association, belong to the specific historical and material sciences. The 'form' in question is reciprocal action, association; and in the most various types of social groups—religious, economic, etc.—we find the same special forms of subordination, imitation, division of labor, etc. The mere fact that phenomena are common to all does not make them social, nor do similarities and regularities established by statistics belong here if each has an individual cause. Not what takes place *in society*, but what takes place *by society*, is the field of sociology. M. Durkheim, though his chief aim is to set forth the methods to be employed in the science, prefaces his extremely suggestive articles by a consideration of what is meant by a social fact. A social fact comprises "every kind of activity or functioning, whether fixed in definite law or not, which is capable of exercising an external constraint upon the individual, or one which is general throughout a given society and has a proper existence of its own, independent of its individual manifestations." Language, law, financial systems impose themselves on the individual. They are general because collective (*i. e.*, obligatory), and not *vice versa*.

2. The *relation of sociology to psychology*, implied in these various definitions, is in many cases evident. None of our writers would, I think, accept Mill's statements, "men are not, when brought together, converted into another kind of substance," "human beings in society have no properties but those which are derived from and may be resolved into the laws of the nature of individual man"—*i. e.*, if we understand by 'individual man' man in isolation from all social relations; and it is difficult to find anything worth saying in the statement if it does not mean this. Small, Tarde and Durkheim lay especial emphasis upon society as more than the sum of individuals, and the last named devotes a special criticism to any psychological explanation of social facts—meaning by 'psychological' an explanation based on the laws of the individual consciousness. Similarly Professor Small would distinguish "between (1) *Psychology* which gives an account of mind as we know it in the individual, and (2) *Social Psychology*, which describes the phenomena that result from the combination and reaction of the cognitions, emotions and volitions of associated individuals." Now, I can but think that Mill's 'individual man' hovers in the background of these statements, and that if psychology is limited to the study of that mythical being, its field is so narrow as to be scarcely worth tilling. No doubt the

sociologists are leading the psychologists into broader horizons; but if we follow out the implications of the authors cited, and abstract from 'mind as we know it in the individual,' all that results from the forces mentioned under (2), *e. g.*, all the effects of language and intercourse, all feelings of sympathy, love, all volitions involving others, we should certainly have a much smaller man than even present psychology studies. Would not the 'difficulty of demonstrating the existence of social as distinguished from individual knowledge, feeling and willing, be removed by the recognition here of what is elsewhere implied in the authors' conception of man, viz., that neither (1) nor (2) deals with anything but an abstraction. There is no 'individual mind,' *i. e.*, mind not under the constant influence of the social relations; neither can we hope to find any 'social' knowledge or feeling, *i. e.*, knowledge or feeling not existing in the medium of individual consciousness. We may doubtless study various aspects of consciousness in isolation, but their abstract character should be distinctly recognized, and neither 'psychologist' nor 'sociologist' can ignore the work of the other.

3. *The methods* to be used in investigation are treated at length by Durkheim. They include (1) rules relating to observation of social facts. These are: (a) consider social facts as things. Bacon's 'idols' all find their counterparts in social science at present; (b) in defining and grouping phenomena, such as crime, the family, etc., use at first external marks only, not by what may be deemed the essential characteristics, nor should merely one type of facts be selected and the rest tested by this standard; (c) study especially the consolidations of social facts in law, proverbs, modes, etc. (2) Rules for distinguishing the pathological from the normal. Here the rather startling proposition is advanced that, at the outset at least, the only objective criterion for the normal is the general. Hence normal=the mean, diseased=the exceptional. But it is evident that greater frequency must ordinarily be due to superiority, to 'health;' hence (a) we may control our results by seeking the cause of the generality of a given phenomena in its relation to the general conditions of life in the social type considered; and (b) this becomes especially necessary in the case of a social species in a transitional stage. (3) For making the classification into social types, the objective principle to be adopted as our standard is that of simplicity—*i. e.*, we ask whether a given group is made up of units which enclose other units more simple than itself. (4) Rules for explaining social faults. (a) It is common to find the reason of a fact in its utility. This is to confound final with efficient causation, and

is no more admissible here than in natural science. Function and cause must be examined separately. (b) The cause of a social fact must always be sought in preceding *social* facts, not in states of individual consciousness [see remarks under 2 above]. (c) The function of a social fact should be sought in the relation it sustains to some social end. (d) The first origin of every social process should be sought in the constitution of the internal social medium. This will depend upon two factors: (α) the number of social units, the '*volume*' of society; (β) the degree of concentration, the 'dynamic density,' which is a function of the number of individuals who are in commercial and social relations. If we do not adopt this plan we are reduced to explain progress by 'tendencies' instead of by real causes; and, further, are forced to treat all as one species in greater or less stages of advancement. (5) Methods of induction. (a) The doctrine of plurality of causes bound up with Mill's philosophic presuppositions is to be rejected. The same effect is *not* produced by different causes. (b) The most valuable of the inductive methods for our purpose is that of concomitant variations, for which we may draw our facts either from a single unique society at different times, or from several societies of the same kind, or from several of different kinds.

The principle most likely to challenge criticism is that reducing the normal to the general. M. Tarde has already criticised (*Revue Phil.*, Feb., '95) Durkheim's own inference from it: that crime must be normal because general. But are not both D. and T. hasty in asserting that the above definition carries with it the inference that crime is normal? Is crime ever so general in any group as to be the rule and not the exception—meaning by crime, of course, acts considered criminal by the group in question?

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NEUROLOGY.

Anatomie des Centres Nerveux. J. DEJERINE, avec la collaboration de Mme. DEJERINE-KLUMPKE. Vol. I. General Methods of Study, Embryology, Histogenesis and Histology-Anatomy of the Fore-Brain. With 401 figures. Paris, Rueff et Cie., 1895. Price, 32 francs.

The revolution in the doctrine on the architecture of the nervous system has been followed by a number of publications which endeavor to do justice to the rapid progress. None of those books reaches in breadth of plan and in the number and choice of illustra-

tions the new work of Professor Dejerine, the first volume of which has just come out. Professor Dejerine, a pupil of Vulpian, is in the midst of a brilliant career as a clinician in the field of nervous diseases. Independent of Charcot's school, he has done remarkable work in the Hospital of Bicêtre, which is probably without rival in rare nervous affections. His wife, Madame Dejerine-Klumpke, of American birth, has become known through her experimental work in Vulpian's laboratory and through her monograph on polyneuritis. The volume before us is the first of a monumental work on the nervous system from the hands of clinicians, and will for this reason be of the greatest intent and practical value.

The first part begins with the history and description of the methods used for the study of the nervous system (pp. 7–57). A very interesting discussion of the choice of a post-mortem dissection, especially in pathological brains, with full description of the author's own method, precedes the notes on hardening, embedding, staining and drawing. The second chapter (pp. 58–153) deals with the development of the nervous system, and contains valuable teratological remarks, besides a remarkably clear and well-illustrated account of the assiduous work of the last years. The third and fourth chapters (pp. 134–232) are devoted to histogenesis and to histology of the central and peripheral nerve elements in the adult. The first part is profusely illustrated with drawings taken from the publications of His, Retzius, Cajal and Ranvier; the selection of the drawings and their execution deserve equally high praise.

The second part of the volume treats of the anatomy of the Fore-Brain. The first chapter (pp. 223–386) covers the general morphology, the convolutions and fissures, the base of the fore-brain and the configuration of its interior. Numerous drawings from photographs take the place of the customary diagrams. The second and third chapters (pp. 387–666) are practically a description of macroscopic and microscopic serial sections through the cerebrum, made in different directions, and forming the most complete atlas of those parts published so far. A chapter on the cerebral cortex (pp. 667–741), and one on the white substance of the cerebral hemispheres (pp. 742–810), form the end of this first volume, rich in pathological observations with regard to the association systems. The second volume will bring the description of the remaining parts and a systematic analysis of the fibre tracts.

The style is very clear, the current epitome on the margin of the pages very convenient. Schematic drawings are avoided; the figures are very accurately drawn and clear.

Thus we have before us the first half of a work of fundamental importance for the progress of neurology, destined to bring the clinician into closer touch with the anatomical literature that is scattered in monographs and journals, and is here for the first time made satisfactorily accessible. Meritorious as are the smaller works of Edinger, Obersteiner Féré, Debierre, and the purely anatomical treatises, none of them could give such a full account of the minute details that interest the clinicians and pathologists of to-day. It is to be hoped that the second volume will soon follow and bring a very accurate index.

ADOLF MEYER.
KANKAKEE, ILL.

The Growth of the Brain in Men and Monkeys, with a short Criticism of the usual Method of stating Brain-ratios. A. KEITH. Journal of Anatomy and Physiology, Vol. XXIX, Part II, Jan., 1895.

The novel data in this paper comprise a series of brain-weights from 135 catarrhine apes. These have been collected from the literature and carefully compiled in tables. For reasons given in the text, brains preserved in alcohol are considered to have lost about 33 per cent. of their fresh weights. Where the brain-weight was deduced from the cranial capacity, the capacity in cubic centimeters was taken as equal to the same number of grammes. There is evidence that the brain much more nearly fills the cranium in these apes than it does in the case of man, and this method introduces no very great error. To be compared with these data are those on the growth of the human brain, based on the figures by Boyd.

The period during which the brain grows is much longer in man than in apes, even though the author is inclined to limit the growing period in man to twenty years. The difference between the brain-weight at birth and at maturity in man is greater than in the apes, or other mammals which have here been studied. This is but a different expression of the fact that in man the brain grows for a longer period. The central nervous system is precocious in its development, and attains nearly its full weight, while yet the body-weight is little more than one-third that of the adult. In all mammals this precocity of the central system is evident, and is most marked in the cephalic subdivisions of it. All these features are emphasized in man. From a comparison according to sex of the data on the brain weights of the several groups of apes, it appears that just as in man, the male has a heavier encephalon.

This is a most interesting discovery. Our author then attacks the difficult problems of the relation between the mass of the body

and that of the central nervous system. He seeks the determination of the 'corporeal concomitant' or the number of grammes increase in brain for each kilo. of increase in body weight.

Assuming this to be a valid relation, the entire 'corporeal concomitant' of the adult man would be but a small fraction of the total weight of the central system. The explanation here offered is questionable, as is also the explanation of the difference in the weight of the nervous system due to sex; making it dependent on the absolute increase in this corporeal concomitant in the heavier male. Detailed tables accompany this paper. H. H. D.

VISION.

Ueber die Anzahl der unterscheidbaren Spectralfarben und Helligkeitsstufen. A. KÖNIG. Ztsch. f. Psych. VIII, 5, 375–380.

Professor König reproduces with more detail the result which he had already announced—that the entire number of different color-tones perceptible in the spectrum by the normal eye is about 165, and that the entire number of distinguishable brightnesses, from the threshold of sensation to the greatest brightness attainable under the conditions of the experiment, is, for white light, 572. The method is a simple one for the mathematician, and the only wonder is that it has not been applied before. If λ and $\delta\lambda$ are the wave-lengths of two monochromatic lights which are just distinguishable from each other in tone, then $\delta\lambda$ is a variable whose value depends upon the value of λ. Within an interval of the spectrum for which λ changes by any fixed unit, the number of distinguishable color tones would be $\frac{1}{\delta\lambda}$, and the entire number throughout the spectrum would be

$$\int \frac{1}{\delta\lambda}\, d\lambda,$$

taken between the limits λ 430 and λ 655, beyond which, in either direction, the color-tone does not change. The value of $\delta\lambda$, at short intervals throughout the spectrum, has been experimentally determined by Uhthoff (*Gräfe's Arch.*, 34, 4, 1); hence the required curve can be laid down and integrated by graphical means. The number of perceptible degrees of brightness is determined in the same manner from the experimental data furnished by König and Brodhun in their investigation of Weber's law (*Sitzungsber. d. Berl. Akad.*, 1888 and 1889); in this case it is necessary to produce the

curve symmetrically somewhat beyond the part which can be laid down from actual measurements; but that can safely be done, because the area taken in by this means is only a small fraction of the whole area, and has therefore only a small effect upon the result. The entire number of discernible brightnesses is in this way found to be about 660. These results differ very little for the eyes of different normal individuals. For Brodhun, who is green-blind, the differences in sensitiveness to change of brightness were within the limit of probable error; in color-tone his number of distinct sensations is about 140. This number does not differ much from that of the normal eye, for the reason that, although his spectrum does not change beyond λ 550, he has a keener sense for change of color-tone in the blue-green region than has the normal individual.

In this connection it may be interesting to point out a most extraordinary statement which occurs in a book which is otherwise an admirable example of good scientific method—Havelock Ellis' *Men and Women*. It is there stated that Newton was able to distinguish seven different colors in the spectrum, but that most people since his time have only been able to see six. To how many of the 165 colors which are actually discernible by the ordinary person, it may be desirable to give a separate *name* for popular use is a question the answer to which may change from time to time; but not to be able to distinguish between this question and the question of the number of colors which can be separated in *sensation*, is to have a mind which is abnormally incapable of drawing distinctions.

The question of the number of differences of saturation which are just perceptible for different colors has been treated by Aubert, Woinow and J. J. Müller; but, of course, the subject lacks all interest when the investigation is not made with spectral lights. To determine by a corresponding method with the one here considered, the total number of distinguishable sensations caused by light of all kinds would require in effect the integration by mechanical means of a solid body in space of four dimensions—what it is not beyond the powers of the mathematician to accomplish.

BALTIMORE. C. LADD FRANKLIN.

Die Wahrnehmung von Helligkeitsveränderungen. L. W. STERN. Zeitsch. f. Psych., VII, 249–278. 1894. *Nachtrag* to the above. Ibid, VII, 395–397.

Die Wahrnehmung von Bewegungen vermittelst des Auges. L. W. STERN. Zeitsch. f. Psych., VII, 321–386. 1894.

A number of researches have recently been published (by Scrip-

ture and Preyer, in Zeitsch. f. Psych., VI and VII) upon the manner in which variations in the rapidity with which a stimulus changes in intensity affects in various senses the perceptibility of the change. In the first of the above papers, with its *Nachtrag*, Stern investigates the same problem with reference to changes in brightness. After summing up the results of previous investigations, which have had to do mainly with the problem of the upper limit, or the conditions under which rapidly successive changes in brightness yield a continuous impression, he describes in detail his own researches as to the lower limit, or the conditions under which a slow (or a single) change in brightness is just perceptible. For these experiments he used a dark box, having on its further inside wall a round white field, which was illuminated by light thrown exactly on it through a lens, and apparatus for increasing or diminishing in measurable amount and rapidity the light passing through the lens, without altering the exact coincidence of its image with the white field on which it was thrown. His results, which he regards as only provisional, are as follows: 1. When the brightening is approximately instantaneous, and is at once perceived, the relative sensitiveness to change of intensity is constant; Weber's law is valid. He found this relative sensitiveness to change to be about $\frac{1}{80}$—not so fine as sensitiveness to simultaneous differences. 2. If the objective change lasts for a short time before it is perceived, the results as to relative sensitiveness and as to duration of objective movement before perception of change, are distinct: (a) The absolute rapidity of brightening remaining constant, the duration before perception of change is greater the greater the initial intensity; within a certain range of intensities the sensitiveness to change remains constant. (b) The relative rapidity of change remaining constant, the duration is shorter, the relative sensitiveness greater, the smaller the initial intensity (and thus the absolute rapidity). (c) The initial intensity remaining constant, the duration is longer and the relative sensitiveness greater, the smaller the absolute rapidity. (This does not apply to sensitiveness in case the perception of change is instantaneous.) 3. Other things equal, changes in brightness are more rapidly perceived and the relative sensitiveness is greater in indirect vision than in direct. 4. The relative sensibility is less ($\frac{1}{2}$ to $\frac{1}{4}$) when the changes become perceptible only after an interval, than when they are perceptible at once. 5. The reaction time in perception of gradual change in brightness is of considerable length (.4 to .7 sec.). Stern believes that the instantaneous perception of change in brightness is different from the perception of change through

comparison of two phases; both often coöperate, but can also appear separately.

Stern's second paper is a monograph on the question of the visual perception of movement. It consists of four subdivisions. The first (pp. 322–328) discusses the (1) facts—upper and lower limit, difference between perception of phases of movement and of the movement itself, perception with resting and moving eye; (2) the characteristics of movement-perception—influence and recognition of direction, of rapidity, of duration, of presence of objects at rest, the sensitiveness to difference for and the attention to moving objects, and the relativity of movement; (3) perception of movements with different portions of the retina; and (4) visual illusions of movement. The second division (pp. 328–341) gives a historical summary of theories. The third (341–352) announces the results of the author's own observations and experiments, showing (1) a confirmation of Exner's observation that the sensitiveness of the retina in the peripheral portions is greater for moving than for resting objects; and that this is due to irradiation, for both irradiation and sensitiveness to movement diminish by diminished illumination; and (2) that after-images of movement appear when the eyes are closed as well as when they are directed upon some resting object, after observing the movement; they are both positive and negative if the eye has been fixated during an appreciable time, positive only if the eye has not been fixated during observation, or has been opened for a fraction of a second only. The fourth division (353–385) discusses the theory of the visual perception of movements. Three theories as to the sensory factors in such perception are evidently possible: (1) that they consist of several successive impressions of the object at rest—the different phases of the movement—and that the fact of movement is inferred from these; (2) that a sensation of movement exists, specifically different from other sensations, fully elementary and unanalyzable, like color or tone (held apparently by Exner, James, etc.); (3) that a single sensory impression, obtainable in a single instant, can yield the perception of movement; but that this sensory impression, instead of being unanalyzable, consists really of a particular complex of sensations from retina or eye-muscles, or both. In other words, it is a sensory group, forming a 'fusion' which is introspectively unanalyzable, but is really separable into elementary sensations of color and of muscular contraction, with differences in intensity, quality and spatial relations. The term 'sensation' is applicable only in the sense in which it is used in expressions like 'sensation of depth, of smoothness,' etc. Stern rightly

rejects the second of these theories and accepts the other two. He finds that when the sensory impression is momentary, admitting of no comparison of phases, it may be of three varieties: (a) 'changed stimulation'—change in intensity or kind of stimulation in a given region of the retina may under certain conditions give the perception of movement; (b) the 'after-image trail' (Nachbildstreifen)—in case of movement of a certain rapidity, the after-images of previous phases are still present when a new phase is reached, and form a series of images of the same object, running into one another, and forming a complete scale of intensities. Thus different phases of the movement are seen simultaneously. Though we are not ordinarily conscious of these after-images, yet they give to the sensory complex its particular nuance. Changed stimulation can inform only as to the fact of movement; the after-image trail can inform also as to what the object is, its direction and rapidity, and makes possible also the simultaneous perception of several movements in different directions. (c) Eye movements. These three principles, together with the two varieties of comparison of phases (optical and muscular), make five sensory factors, any one of which may give the perception of movement, but which also form varied and complicated combinations with one another. Stern, in conclusion, applies these principles to the explanation of certain complicated perceptions and illusions: the reversibility of the impression of movement, the relativity of movement, the rapidity and after-images of movement.

Stern nowhere discusses the part played by memory-images of other sensory factors, when any one of the above-named factors arouses the perception of movement. He claims, for instance, that the Nachbildstreifen alone, independently of eye-movement sensations, can give the perception of movement. It is true that this may be the only present sensory factor. But the complete perception of movement must be a very complicated affair, the gradually perfected product of a great amount of experience of coöperating sensory factors; and may it not be that the memory-images of sensations from eye and other active bodily movements form a prominent and necessary factor in every perception of movement? If so, then the Nachbildstreifen or other singly present sensory factor, even when it arises from several simultaneous movements in different directions, would receive its perceptual interpretation as movement only through the admixture of such memory-images.

Furthermore, to the reviewer, it has seemed as if there might be a sixth sensory factor sometimes operative, especially in case of the

after-images of movement. When the eyes have been exposed for a length of time to a flickering light, the flickering sometimes continues for a period after cessation of the stimulus; and might be explained as a continuation of the rhythmic adjustment which the retina during stimulation is making to the rhythmically changing intensities of stimulation. Now in case of prolonged observation of a movement, as of a revolving spiral or a waterfall, etc., such flickerings practically proceed in waves along the stimulated portions of the retina, and may possibly continue in the same or opposite sense on cessation of the stimulus, and be a factor in the positive and negative after-images of movement, which Stern attributes wholly to the Nachbildstreifen.

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MEMORY AND ATTENTION.

Experimentelle Untersuchungen über das Gedächtniss. W. LEWY. Zeitschr. f. Psych. VIII, 231–272.

Of the two groups of experiments here recorded, the first relates to memory (retentiveness) of small visual distances (20–200 mm.). By the use of an appropriate 'Augenmassapparat' on which the 'normal' distances were each exposed for 5 secs. in arbitrary order and by application of the method of average error, it was found that the error increased, in general, with the time of retention (1–60 secs.), with exclusion of ocular movement, with distraction of the attention during the interval of retention, with shortened exposure-time, and when two distances for retention were taken in succession instead of one. The most striking exception was that one second for retention was much less favorable than two, due, in L.'s opinion, not to oscillating attention, but to the necessary haste. Other variations suggesting periodicity in the clearness of the image, he inclines to attribute to causes as yet unknown. The memory was not appreciably improved by practice.

The second group of experiments deals with the local memory of simple touch-sensations. The area of stimulation was a limited region on the dorsal side of the arm above the wrist, the method of measurement again that of average error. Under the most favorable conditions it was found that the error steadily increased with the time-interval for retention; was more frequent and greater in a distal than in a proximal direction; was less when estimated with reference to the point ultimately judged right than when referred to the point first selected; curiously varied, being even in one case

less, when the retention-interval was 'filled.' The slight influence of mental reckoning compared with its marked influence in the case of small visual distances, indicates the importance in reckoning of ocular movement. Accuracy of localisation was improved with practice, with variations, however, not easily explained. The introspective evidence, which is well presented as supplementary to the bare record of the tables, shows that the feeling of defective attention by no means always coincides with greater error in localisation, while in the first group it shows how manifold and individually varied are the factors which determine the accuracy of even a comparatively simple act of recognition.

The Relation of Attention to Memory. W. G. SMITH. Mind, N. S. IV, 47–73. January, 1895.

An experimental study from the Physiological Laboratory at Oxford! Under suitable conditions sets of letters were exposed each for 10 secs.; the reagent then repeated what he could remember. While memorizing, the attention was variously distracted. Finally, experiments were made for 'normal' results. Comparison of the cases showed that the greatest disturbance was caused by the activity involved in summation (arithmetical progression), and that that produced by speech (repetition of a syllable) was greater than that produced by mere muscular movement (tapping with the finger). The results emphasize afresh the importance of the motor factor in memory, particularly in the suggested interference of the articulatory innervations involved in memorizing by the activity of the vocal mechanism. In the relatively large number of errors of insertion and displacement in the group where distraction was greatest, the author sees the influence of inattention not only to cause fewer ideas to be recollected, but especially to confuse and derange the associative connections: this against Münsterberg. Inattention disturbs the apperceptive process, tends to turn *Wahrnehmung* into *Empfindung* and to produce a sort of *Seelenblindheit;* the essential fact in attention, on the other hand, is 'the strengthening of an idea or impression by the processes of blending and redintegration.' Contiguity in association is merely formal; the real causes are dynamic factors, such as Attention and Interest, and these mainly in connection with motor agencies.

The author introduces a novel 'positive' method of measuring the accuracy of reproduction, viz., by marking it on a certain scale like an examination-paper; but it does not appear to what extent the 'negative' method of error, which he also uses, is thereby con-

trolled. The paper as a whole represents a noteworthy transition from the experimental study of the time-relations of memory begun by Ebbinghaus to the more difficult experimental study of the analysis and dynamic relations of its constituents.

What do we mean by Intensity of Psychical States? F. H. BRADLEY. Mind, N. S. IV, 1–27. January, 1895.

That sensations are not measurable quantities, that "our judgments of more intensity can be expressed without the hypothesis that more units have been added to a growing sum" (James), is an opinion now widely prevalent as the result of innumerable discussions of Fechner's psycho-physical formula. Such is not the opinion of Mr. Bradley. He holds that not sensations only, but psychical states generally, have quantity in all manner of respects, are in principle measurable, and therefore, since degrees not resting on units are meaningless, do, in some sense, imply the hypothesis which the above quotation rejects. The argument, which is too subtle and elaborate for brief reproduction, admirably succeeds in its professed object of raising doubts. In the exposure of ambiguities, the analysis of aspects, in fact, the whole dialectical 'business' of discovering and considering difficulties, Mr. Bradley shows his unrivalled skill: there may be much more to say on the points noticed, it would be hard to name any point essential to the discussion which has been overlooked. The article is too important to be passed without comment, especially by psychologists committed to an opposite theory, who in propositions like the following, 'the idea of the extended has extension, the idea of the heavy has weight,' etc., will no doubt find matter for explosion. On the other hand, the views which Mr. Bradley recommends would seem to have little bearing on the actual measurement of states of consciousness, so far, at least, as they are considered as amounts of psychical existence; for here the units, though they must be assumed, cannot probably be shown. Relativity of strength or amount, distinctions of kinds and scales—these are the leading ideas, the import of which may be seen in the confusion which disregard of them has brought into many a controversy.

An Analysis of Attention. ALEXANDER F. SHAND. Mind, N. S. III, 449–473. October, 1894.

The confusion complained of by Mr. Bradley has certainly not been wanting in the controversy as to the effect on the intensity of mental states produced by attention. Attention is said to increase

the strength of sensations and the clearness of ideas, and in general to be connected with predominance in consciousness of the presented content; with which assertions the view that attention is not itself presentable but a 'special activity' of the subject is sometimes associated. Mr. Shand denies the necessity of this supposed connection of attention with increased intensity or clearness in the object: the object may become obscured or even fade out while we watch it—a remark which seems also to deny that the so-called fluctuations of attention need be fluctuations of attention at all. What attention really does is to make us more clearly aware of the object, to make our consciousness of it predominant, or rather, since 'attension' merely expresses the fact 'I am attending,' it *is* this clearer awareness. The changed strength of the object is primarily due to variable concomitants of attention, such as accommodation and 'interest'; attention itself is a distinct process. This process consists in apperceiving a felt content in such sort as to develop a greater awareness of its systematic complexity. Its earlier stages actively condition the later; it also reacts, the duality of consciousness being after all a 'continuum,' on the idea or sensation attended to, so far, namely, as to make that more active in evoking the fusion and association necessary to the further understanding of the object. Thus one element in attention is 'feeling,' immediate awareness of presentation, the other is 'thought' or interpretation. And this process, *pace* Mr. Ward, is as directly felt or experienced as sensation.

That I am or may become aware of the degrees of my awareness, and that attention, in the sense defined, is therefore a fact of experience and not a metaphysical abstraction seems indisputable. How far, on the other hand, it is rightly said to be directly felt, may be a question of terminology; but in asserting this direct feeling of a distinct process as over against the feeling of what are described as its concomitants, *e. g.*, the strain of accommodation, etc., Mr. Shand is virtually engaged in a triangular combat—he opposes not only Ward, but some to whom Ward is himself opposed.

SMITH COLLEGE. H. N. GARDINER.

ETHICAL.

La Sanction Morale. F. PAULHAN. Revue Philosophique, Mars, 1894, pp. 267–286, and Avril, 1894, pp. 395–419.

According to the author, the Moral Sanction is the logical consequence of responsibility. Responsibility is merely a certain apti-

tude for the sanction. The moral tendency of the first is satisfied by the second, in much the same manner as the tendency of hunger is satisfied by the act of eating. To regard a sanction as logical and moral, implies a logical and moral world perfectly systematized, and which is realized in our world in proportion as that world is moral and logical. The consequences of the acts of an individual, which constitute this moral sanction, may affect the individual himself if the personality regarded as a whole is responsible, or merely the tendencies which have determined these acts; but always in a way calculated to lead to a more complete systematization of the whole, an element of which experiences the sanction. The only end and the only justification of punishment or of reward, the essential elements of the sanction, are the elimination or prevention of evil, the furtherance or the development of the good. Pleasure and pain are signs of the sanction rather than the sanction itself. Repression of evil is in itself a sign of disorder; in a perfect world, devoid of evil, punishment would be wanting and the sanction would consist merely in the preservation of the organism.

As regards partial and impartial responsibility and the corresponding sanction, inasmuch as there is always a certain solidarity between the different parts of a moral person, the responsible element can be reached ordinarily only by acting upon the individual or upon another element. The more this element is systematically associated with the individual, the more the sanction applied to the individual will result in coördinating the elements of the ego, so that they in turn will exercise their combined influence upon the responsible element, and consequently the more the reward or punishment of the individual will be just, and will have the character of a moral sanction. However, inasmuch as the complete coördination, or total incoördination of the psychical elements are purely theoretical cases, no sanction affecting the entire personality is ever applied either with absolute justice, or absolute injustice.

The general rules of the sanction apply also to the diseased, to the insane as well as the sane. So far as parts of their minds, some tendencies, may still afford some degree of coördination, they may be the object of a moral sanction. In the great majority of cases the sanction should not be applied to the whole of the personality; consequently tribunal intervention would be uncalled for. The sanction applies vigorously to criminals who are called insane only because they lack altruistic, or moral feelings, provided the coördination of their acts and feelings is complete and consistent in all other respects.

The sanctions of the social organism are akin to the sanctions of

the psychical, and the two follow a similar process of development, in which three distinct stages may be noted. The first and inferior form of sanction is automatic in character, and arises where the systematization is but slight; the final form is also automatic, but superior in this that the systematization is far greater. There is an intermediary stage of a conscious form of sanction, where preparation is made simultaneously or successively for the superior automatism. This is accomplished by the growth and synthesis of the new elements which are to enter into the final stage, and by the coördination of the acquisitions already made, and also by the more active intervention of the social ego which has not regularly interposed in the first stage, and which in the final one is represented by the general solidarity of the elements of the system. The sanction is more perfect in proportion as the good is more simply encouraged and the evil more simply restrained, and the greater, the precision, without employing intermediaries merely to apply the sanction. The penal sanction is therefore indirect and incomplete. The perfect natural sanction, without intention of reward or punishment, is the best and highest form. F. Paulhan, in short, regards the moral sanction as a particular case of natural selection, in the conservation of the good, and the elimination of the evil. It seems to me, however, that the conscious feature of the intermediate stage in the development of the moral sanction from inferior to superior automatism, implies a voluntary psychical factor which is quite foreign to natural selection. In this stage would appear reflection, deliberation, conflict of desires, the various manifestations of moral feeling, etc., all of which must either be explained away, or their importance unduly minimized if F. Paulhan's account is to stand. Moreover his designation of sanctions as just, or unjust, introduces ideas whose metaphysical implications are incompatible with a naturalistic ethic.

Origines et conditions de la moralité. PIOGER. Revue Philosophique, Juin, 1894. Pp. 634–656.

The author contends that there is a difference between a theory of morals and moral practice. The latter antedates the former and gives form and content to it. The early moral, and religious practices as well, have preëminently a social character. Even the conception itself of moral good implies the conception of a social good. Morality is not merely a matter of the intention; and even if it were, the intention must be regarded as having very complex antecedents, and as a part of an extended system showing a solidarity in the act and in the intention, and a still more complex solidarity

of act and intention taken together. Morality is rather the product of sociability, producing social instincts, appearing in the form of customs, moral practices, and finally a theory of morals. As health is the harmony of organic functions, morality is the harmonious manner in which the reciprocal relations of social beings is established. Immorality is social disease.

There is no intuitive moral law before experience, for there is experienced morality before the consciousness of moral law began to dawn. This is abundantly established by the marked sociability among animals and an unbroken line of development from the social instincts of the lowest order of animals to the reflective morality of the most highly civilized man. Morality is, therefore, the result of the social and moral evolution of the race. The unconscious solidarity of social animals, the morality of the Fuegians and Tasmanians, and the advanced morality of our age, all have the common elements of reciprocity and mutual dependence. Even in the life of to-day conscious morality is the exception, and automatic, instinctive morality is the rule. Reflection, questionings of conscience, deliberations of the will, all mark a derangement of the natural moral functioning. Therefore, since morality is developed, it is necessary to go to the simplest and earliest forms of its manifestation to discern its essential character. Beginnings of morality emerge in the necessities of hunger, defense, reproduction, etc.; this is true also for all social animals. In history morality develops parallel to social organization. The force of obligation and of moral sanction lie in the appreciation of the end — conservation of the species and the race. Intelligence becomes more and more aware of the necessity and advantage of solidarity. Moral sentiments are accounted for, inasmuch as the organic functional character of morality is such as to form an integral part of our social vitality, and cannot be disturbed or inhibited without results disturbing it and the unity of action of all our functions. The genesis of social conscience arises through a differentiation of the nervous system especially adapted to the reception of social excitations, as the sense of sight results from a nervous differentiation especially adapted to receive luminous vibrations. Morality is conformable to the same general laws of differentiation, and of coördination, and adaptation and organization, as our other forms of physiological and psychological activity.

Dr. Pioger, as it will be seen, takes a point of view similar to that of F. Paulhan. His article contains also several unwarranted inferences. While it is true that the lower order of animals, the

morality of the Fuegians, and that of the most highly civilized peoples, may contain the common element of sociability, still that common element represents a minimum which is quite inadequate to express completely the essential features of morality. Prof. Edward Caird's contention in reference to the evolution of religion has a similar application to ethic: "that we must read development backward and not forward, and that we must find the key to the meaning of the first stage in the last; for to trace a living being back to its beginning, and to explain what follows from it by such a beginning, would be simply to omit almost all that characterizes it, and then to suppose that in what remains we have the secret of its existence." While morality may be necessarily concerned with social relations, it may quite as well give law to qualify and transform these relations, as that these relations should determine wholly the character of the emerging laws. That moral practices existed before formulated laws does not prove that the laws were not existent, implicitly at least; nor can it be inferred that the laws were merely a classification or a summation of these practices.

The Method of Idealist Ethics. H. MELLONE. Philosophical Review, January, 1895. Pp. 47–64.

The most fundamental ethical question is the following: What is the supreme Ideal of human life? An answer to this implies an answer to other questions dependent upon it,—the meaning and significance of moral authority; the ultimate criterion of morality in conduct, the connotation of the conception of right; and the proximate criterion, the denotation of right. The question of there being an ultimate ideal is an ontological one; it is the question of the nature and purpose of the individual life. The ideal is regulative, not in the sense of showing us what is to be done, but that *something must be done.* Ideas of the concrete forms of duty come from sociological and historical studies, which, however, belong rather to the sphere of practical or applied ethics. In the analysis of our judgments we find some depending on a standard of truth, but others depending upon a standard of value. The latter from two classes; one, which is formed independently of the will, æsthetic judgments; the other, are ethical judgments depending on a standard of right; that is, on a meaning or purpose in our lives. The motive which prompts us to seek for standards of value in these three respects is experienced under the form of feeling. The standard is felt as an obligatory ideal; in thought, as an ideal of truth; in conduct and character, of goodness; in (creative) art, of beauty. Natu-

ralism or Materialism cannot explain these ideals. In them Idealism finds a key to the nature of the Absolute. Feeling is the fundamental medium of connection and communication between the individual and the universal consciousness. Purposive action is feeling-prompted action. This induces a striving or ἔρως in our nature of a three-fold character, corresponding to the three ideals—truth, goodness, beauty. From the individual point of view this striving is after what is not yet realized, but may be so—what is potentially ours. From the universal point of view, these feelings, as they tend to become supreme, constitute a self-surrender to that which is externally real. A process in time cannot be the ultimate and most fundamental fact in the universe. In so far as the absolute is such a process, or has a history, its essential nature is not manifest. However, it is necessary to keep firm hold on the reality of the time-processes of growth and change in the individual lives for which the ideal may be more or less fully realized. Here we are confronted with the problem : In what sense is time a reality ? Time or change is neither an absolute reality nor an absolute unreality. There must be some *via media* between them, which makes it possible to conceive of reality as a multiplicity of individual, finite, growing lives, immanent in a universal and eternally complete life.

The author's comment at the end is in itself the most appropriate criticism of the article as a whole:—"this is not to attain to a full explanation, but only to begin to see the possibility of one." He is to be commended for his insistence upon a distinct selfhood of the individual, however, immanent in the universal life.

JOHN GRIER HIBBEN.

PRINCETON.

PATHOLOGICAL.

On some of the newer Aspects of the Pathology of Insanity. W. L. ANDRIEZEN. Brain, Part LXVIII, winter 1894.

Owing to many improvements, to which the author himself has been an active contributor, the silver staining method of Golgi has been brought sufficiently under control to enable the detailed anatomy of the central system to be studied by its aid, and to warrant its application to pathological material.

This paper of one hundred and fifty pages attempts a statement of the fundamental plan underlying the arrangements of the nerve cells in the central system ; the expression of this plan in the cerebral cortex ; an examination of the cortex in the vertebrate series, and by this means the determination of the highly elaborated arrangement in man, and the separation of those parts which are

fundamental from those which have been later added, and which constitute the important distinctions between this most complex cortex and that of lower vertebrates.

To all points of the cortex impulses come in over afferent pathways and from lower centres, and leave by efferent elements whose cell-bodies form part of the cortical layer. In this manner all parts of the cortex, where radiations of sensory pathway are found, become turning points, at which such incoming impulses are transmuted into those outgoing. At some of these points, it appears, that sensations may follow an experimental stimulation of the cortex itself, even though the stimulus be insufficient to give rise to a well marked efferent impulse with its associated reactions, and in this we see, not only a confirmation of common experience, but also warrant for the inference that in the upper layers of the cortex are to be found the structures whose activity is accompanied by consciousness. In confirmation of this general conclusion there is found a greater differentiation of the outer portion of the cortex in the predominantly sensory regions—as shown, for example, by the development of Gennari's stripe in the visual area.

The cortex of man is most different from that of other vertebrates by reason of the great development of the deepest layers of cells. All observations point to these cells as concerned in associating the cortical areas one with another, and we thus have, as contrasted with other vertebrates, an unusual anatomical development in man to be associated with the unusual form of mental activity in man. Passing on to the application of these facts to disease arising from disturbances in the cortex, A. finds in cases of alcoholic insanity a degeneration of the dendrons, and even of the cell-bodies of the larger cells constituting the middle layer in both the Rolandic and occipital areas, changes which, in the first instance, would render the passage of the impulses from the incoming fibres to these cells more difficult; and in the second, when the cell body itself is involved, must modify and weaken the subsequent discharge from it, thus giving a good anatomical basis for the symptoms observed. The changes just mentioned, however, are extreme, and between cells so altered, and those in full health, all gradations may be observed.

Throughout the paper the anatomical point of view is that which has been recently worked out by the more advanced investigations in this line, and need not be here recapitulated. In the course of the argument many matters are discussed, but the paper is most important by reason of the good histological evidence here offered for

the architecture of the cortex, and most interesting to psychologists because of the attempt to correlate this architecture with some fundamental features of brain activity. H. H. D.

La Psychologie de l'Amour. G. DANVILLE. Paris, Alcan, 1894. Pp. 169.

M. Danville (the pseudonyme by which a brother of one of our most distinguished neuro-pathologists conceals his identity), is already known in France by his stories, and by a curious article published in the *Revue Philosophique:* 'Is love a pathological state?' (March, 1893). His present book broaches a question which until now has never been scientifically treated, although the way has been cleared by previous works by Stendhal, Balzac, Schopenhauer, Mantagazza, and Bourget. M. Danville gives us nothing that can be properly called the psychology of love as founded on observation and testimony (which would be a very useful work). What he gives us is a definition of love. Love is (1) a *specific, entity-like motive*, distinct from tenderness, sympathy, pleasure, benevolence, etc.; (2) subject to exclusive systematization, with consciousness of sexual desire toward a definite person of the other sex; that is to say, in less barbarous terms, that real love is monogamous; (3) accompanied by exaltation of sexual desire. Love is not itself sexual desire, the latter is only an accompaniment or an effect; (4) it is accompanied by special mental processes. As a phenomenon of consciousness love may be described in the following way: It forms in each of us, by successive perception of those who awake the sexual instinct, a latent subconscious image which sums up all our preferences; it is for a man an ideal image of the most perfect woman; it is something like Galton's composite image, developed in connection with a particular sense. This image persists through a long time, aided by any daily attention which it may receive. Every normal adult thus possesses within him such a subconscious synthesis which is nothing else than his latent power of loving, to be brought out when any real being approximates its characteristics closely enough to call it into activity. It is a curious and, possibly, new theory; and is the only thing in the book of which as much can be said. A. B.

PARIS.

NEW BOOKS.

Substance and Attributes. ANON. London, Kegan Paul, French, Trübner & Co, 1895. Pp. XV + 197.

The Foundations of Belief. A. J. BALFOUR. New York, Longmans, Green & Co., 1895. Pp. VIII + 366.

Gehirn und Seele: Rede des Rectors. P. FLECHSIG. Leipzig, Edelmann, 1894.

Dictionnaire de Physiologie. CH. RICHET. Tome I, Fasc. 1, A—Ali. Paris, Alcan, 1895. Pp. XII + 336. 8 fr. 50.

The Gospel of Buddha. P. CARUS. Second ed. Chicago, Open Court Pub. Co., 1895. Pp. XIII + 275.

La vie et la pensée. T. PIOGER. Paris, Alcan, 1895. Pp. 260.

Introduction to the Theory of Science and Metaphysics. A. RIEHL. Trans. by A. Fairbanks. London, Kegan Paul, French, Trübner & Co., 1894. Pp. VIII + 346.

Dégénéré's et Desequibrés. J. DALLEMAGNE. Brussels, Lamertin; Paris, Alcan, 1895. Pp. 658.

Entartung und Genie: Neue Studien. C. LOMBROSO. (Deutsch von Kurella). Leipzig, Wigand, 1894. Pp. 308.

Degeneration. MAX NORDAU. New York, Appletons, 1895. Pp. 560.

Thoughts on Religion. G. J. ROMANES. Edited by Charles Gore. Chicago, the Open Court Pub. Co., 1895. Pp. 184. $1.25.

The Philosophy of Lotze: the Doctrine of Thought. H. JONES. New York, Macmillan & Co., 1895. Pp. XVI + 375.

John Stuart Mill. C. DOUGLASS. Edinburgh and London, Blackwood & Son, 1895. Pp. XV + 274.

NOTES.

Dr. D. Hack Tuke, the well-known writer on mental diseases and editor of the *Journal of Mental Science*, died in London on March 5.

The current issue of the *Princeton College Bulletin* (Vol. VII, No. 1) is devoted entirely to memorials of the late ex-President James McCosh. It contains articles by President Patton, Prof. A. F. West, and Prof. W. Libbey, Jr., a poem by Robert Bridges, a portrait, and a useful bibliography of Dr. McCosh's writings (over eight two-column pages) compiled by J. H. Dulles.

Dr. Ernst Mach, professor of physics in the University of Prague, has accepted a professorship of philosophy in the University of Vienna, and will direct a laboratory of experimental psychology.

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